Effects of Socioeconomic-Based Affirmative Action: A National Study of Admissions at Selective Public High Schools*

Rene Crespin and Tyler Powell †

September 2024

Abstract

Selective educational institutions have long sought alternative admissions policies to improve campus diversity, while maintaining a high level of academic achievement. Although much of the recent national discourse has focused on higher education, the issue is equally contentious and longstanding at selective K-12 schools. In this paper, we document the persistent under-representation of low-income, Black, and Latino students in academically selective high schools (ASHSs) across the United States over the past decade. Next, we leverage geographic and temporal admissions policy variation to provide the first national empirical evidence on the effects of socioeconomic (SES) class-based admissions policies on the SES/racial diversity and college readiness of ASHS cohorts. Our findings show that class-based AA is effective—significantly increasing the enrollment of low-income students in treated schools compared to status quo admissions at control ASHSs. Indirectly, this policy change boosts the enrollment of underrepresented minority students, primarily at the expense of White students. Lastly, we find suggestive evidence that, on average, the reforms led to minimal or no change in the college readiness of treated ASHS cohorts.

*We thank Maria Fitzpatrick and Scott Imberman for helpful comments and discussions. The research reported here was supported by the Institute of Education Sciences, U.S. Department of Education, through Grant R305B200009 to Michigan State University. The opinions expressed are those of the authors and do not represent the views of the Institute or the U.S. Department of Education.

†Crespin: Department of Economics, Michigan State University; crespin@msu.edu. Powell: Department of Economics, Michigan State University; powel282@msu.edu

Introduction

Selective educational institutions provide students with access to learning environments that are competitive, well-resourced, and composed of high-achieving peers (Chetty et al., 2023; Dobbie & Fryer, 2014; Finn & Hockett, 2012). Long-lasting disparities in access to these institutions call into question the proper balance between selectivity and diversity along racial and economic lines. While recent national discourse has focused on this debate at the university level, it is equally controversial and longstanding in K-12 districts with academically selective public schools.[1]

Selective K–12 schools are often regarded by districts as a source of pride and a means to attract and maintain engagement with the public school system, offering advanced courses and extracurricular opportunities that typical schools may not provide (Finn & Hockett, 2012; Hoxby, 2003). These schools frequently play a role in families' educational decisions, with over 60% of eighth graders applying for a seat in some cases (Feerick Center for Social Justice, 2019; Perez, 2018). Consequently, administrators are incentivized to uphold the schools' strong academic reputations while simultaneously addressing the demands from various stakeholders to increase their diversity (Cestau et al., 2017; Ellison & Pathak, 2021). In response to criticism of the lack of diversity at selective schools, an increasing number of districts have begun to use race-neutral socioeconomic (SES) class-based admissions policies. Despite the growing interest in class-conscious admissions reforms, research on their impacts on school diversity and college readiness is limited.

In this paper, we provide the first empirical evidence on the effects of SES-based admissions policies on the economic/racial diversity and college readiness of academically selective high schools (ASHSs) across the United States, relative to status quo ASHS admissions. While most selective public schools do not change their admissions policies frequently, if at all, we leverage geographic and temporal variation in observed admissions policy changes to analyze their impacts.

To explore this topic, we create a novel national dataset of selective public school admissions policies and their changes over time. We first gather data on the availability of ASHSs since 2010. Next, we use public records to search for changes in admissions policies at each of these schools. Then, we link these data with national school-level records that capture demographic shifts over time.

Using this newly compiled dataset, we first document geographic variation in academi-

[1] In this paper, we use the general term academically selective public schools. In practice, these schools are referred to by various names across districts. For example, Boston refers to them as Exam Schools, Chicago refers to them as Selective Enrollment Schools, and New York City refers to them as Screened or Specialized Schools.

cally selective public high schools and patterns in their representativeness by socioeconomic status and race/ethnicity over time. These selective schools are located across many parts of the US, especially in large urban school districts that serve a significant fraction of the US student population. We show that low-income, Black, and Latino students are underrepresented at ASHSs compared to their representation at non-ASHSs in the same districts. Meanwhile, ASHSs overrepresent higher-income, Asian, and White students. Over time, low-income students have slowly increased as a share of ASHS enrollment across the US, but the share of underrepresented minority (URM) students has declined over time.

Next, we leverage the staggered rollout of policy changes in our national dataset to study the demographic effects of admissions policies intended to improve SES diversity at ASHSs. About 30% of our final analytic sample instituted this type of policy. Policy implementation may not be random in our context if selective schools change their admissions policies in response to new social pressures within their districts, or because of stagnation or deterioration in their diversity. Therefore, our preferred empirical approach accounts for school-specific trends prior to the policy change. Event studies provide evidence that these admissions policy updates led to significant changes in the demographic composition of ASHSs, making it clear that our estimates represent policy effects rather than the continuation of a pre-existing trend.

Using a difference-in-differences (DID) framework that accounts for school-specific pretrends, we find that, on average, class-based affirmative action (AA) policies lead to a 14pp (40%) increase in low-SES enrollment at selective high schools, compared to status quo admissions policies at control schools. We show that these policies also lead to an 8pp (18%) increase in URM enrollment shares, which is almost entirely driven by a 5pp (17%) decline in the enrollment share of White students. On average, these policy changes have little to no impact on Asian enrollment shares at selective schools.

Admissions policies aimed at increasing the representation of low-SES students can be implemented using proxies that target the desired population. In our data, we identify four distinct SES-targeting policies: Low-Income Quotas require a fraction of admitted/enrolled students be from low-SES backgrounds; Eligibility in the Local Context (ELC) guarantees admissions to a percent of the best performing students from each middle school within the district; Priority Groups target admissions benefits based on a student's background and/or lived adversity; and Submarginal Conditional Acceptance (SCA) admits low-SES students who score marginally below admissions cutoffs and successfully attend a bridge program between 8th and 9th grade.

We find suggestive evidence of differential impacts between these different versions of SES-based AA. All four admissions policy types lead to increases in free-lunch and URM

enrollment shares. These impacts on URM are driven by corresponding declines in non-URM enrollment shares, which vary by policy type. Low-Income Quotas and Priority Groups lead to significant declines in White enrollment shares, ELC leads to noisy declines in Asian and other race enrollment shares, and SCA only leads to declines in other race enrollment shares.

Lastly, we find suggestive evidence that, on average, SES-based admissions policies have minimal impact on college readiness, even though they lead to the admission of students who enter with lower academic performance. Our findings support hypotheses from simulation-based research which predict positive academic impacts of exam school admission, especially for applicants whose ASHS assignment status is altered by the admissions reforms.

Relationship to the literature. We make several important contributions to the literature. First, we contribute to a growing strand of research that examines the potential impacts of different admissions policies on the socioeconomic and racial/ethnic diversity of academically selective pre-collegiate schools. This question is increasingly important as educational institutions around the country are shifting away from purely achievement-based or race-based admissions to more race-neutral policies that prioritize low-SES students.

Despite this trend, research into the potential impacts of different admissions policies on selective K–12 school diversity has been limited to theoretical and simulation-based studies. Generally, these studies find that SES-based AA and ELC admissions policies increase the enrollment shares of low-income students by 39% to 74% and of URM students by 63% to 147% compared to purely merit-based admissions (Cestau et al., 2017; Corcoran et al., 2018; Ellison & Pathak, 2021; Rucinski & Goodman, 2022).[2] While all four studies provide important theoretical insights, they must rely on assumptions about families' preferences and admissions dynamics. We test their hypotheses by employing real-world policy variation.[3]

Second, studies that have examined the impacts of AA policies on the demographic composition of ASHSs have done so by focusing on ASHSs within individual school districts. More specifically, they have focused on two of Chicago's Selective Enrollment High Schools

[2]More specifically, Corcoran et al. (2018) and Rucinski and Goodman (2022) show that ELC policies in Boston's Exam High Schools and New York City's Specialized High Schools would be highly effective at increasing low-income and URM enrollment shares at the cost of Asian and White shares. Similarly, (Cestau et al., 2017) show that using FL-based AA would increase free-lunch and Black enrollment shares in an anonymous school district, compared to no AA. Furthermore, Ellison and Pathak (2021) predict that in two Chicago ASHSs, using free-or-reduced-price-lunch (FRPL) status as a direct measure of SES is more effective at increasing FRPL shares compared to using neighborhood-SES as an indirect proxy, but both policies affect URM representation similarly. Additionally, Cestau et al. (2017) and Ellison and Pathak (2021) predict that directly using race-based AA has the largest impacts on URM representation compared to other AA policies.

[3]This study also complements a large and growing literature that examines access-oriented admissions policies on US universities' economic and racial composition. Those studies focus on the impacts of ELC and Holistic Review admissions on lower-income and URM undergraduate enrollment in California and Texas (Black et al., 2015; Bleemer, 2023; Cortes, 2010; Kapor, 2020; Klasik & Cortes, 2022).

(Ellison & Pathak, 2021), New York City's Specialized High Schools (Corcoran et al., 2018), and Boston's Exam High Schools (Rucinski & Goodman, 2022). We provide the first study of admissions policies at the national-level. Among ASHSs in 47 school districts and 23 states, we observe 60 control ASHSs in 42 school districts and 28 treated schools within six districts.[4] This enables us to examine differential impacts from alternative AA admissions policies, allowing for variation in how different schools and districts may implement such policies.

Third, we contribute to a growing literature examining the distortionary impacts of admissions policies on student achievement at ASHSs. Corcoran and Baker-Smith (2018) use simulations to show that although an ELC policy would increase low-income and URM enrollment shares, this would come at the cost of 2% to 12% lower average incoming student achievement. Similarly, Ellison and Pathak (2021) show that, compared to race- and FRPL-based AA, admissions policies that distribute seats based on the student's neighborhood have negligible impacts on FRPL enrollment shares but lead to declines in incoming student achievement. While those studies focus on students' incoming achievement levels, they make no predictions about the eventual performance of students once they enroll in an ASHS.

Prior studies using regression discontinuity (RD) in Boston, Chicago, and New York suggest that ASHSs have negligible impacts on marginally admitted students' college readiness (Abdulkadiroglu et al., 2014; Angrist et al., 2023; Barrow et al., 2020; Dobbie & Fryer, 2014). However, Rokkanen (2015) uses simulations based on an RD extrapolation to show that while income- or race-based AA attracts lower-achieving students, it also increases the 10th grade achievement of inframarginal students who gain admissions through such policies. While our data does not allow us to directly examine the impacts of AA on newly admitted students' academic growth, we are able to explore within-school changes across cohorts' college readiness. We find that while SES-based AA policies increase FL and URM enrollment shares, they do not significantly affect ASHSs' average college readiness. If the SES-based AA policies are admitting lower-performing students who were previously not meeting the performance thresholds and the average college readiness does not significantly change, it would suggest that ASHSs can have positive academic effects on disadvantaged students, which is in line with Rokkanen (2015).[5]

Roadmap. This paper is organized as follows: Section 1 discusses the concept of selective schools and the institutional background of SES-based affirmative action admissions policy changes. Section 2 describes the data collection process and selection of the analysis sample.

[4]For this statistic, we count all NYC school districts as a single district.

[5]In related work, (Black et al., 2023) show that students pulled-in to a selective college through the Texas Top 10% see increases in likelihood of college graduation.

Section 3 describes the empirical approach, while sections 4 and 5 present the main results. Finally, section 6 concludes.

1 Institutional Background

1.1 Where and What are academically selective public high schools?

Academically selective high schools (ASHSs), a form of magnet/choice schools, are viewed by local politicians and school district administrators as a way to attract and maintain engagement with middle- and upper-middle-class families in public school systems that are generally low-income or low-achieving (Finn & Hockett, 2012; Hoxby, 2003). Figure 1 shows that these schools exist in many regions of the United States. Consistent with the idea that they are a recruiting and retention tool in low-income areas, they are mostly concentrated within large, urban districts.

While all schools included in this study are academically selective in some way, their methods for admitting incoming cohorts vary widely. This is exemplified by two examples from different sides of the selection spectrum. Stuyvesant High School in Manhattan is a famous example of a school that takes the exam school moniker literally in its admissions system—basing admissions solely on an exam score (Stuyvesant High School, 2023). Stuyvesant is part of a subset of New York City public schools that are called Specialized High Schools.[6] The New York City Specialized High Schools admission test (SHSAT) is offered to 8th and 9th grade students each year, and students are placed in rank order based on their scores on the exam (Dobbie & Fryer, 2014). Given the distribution of scores and the number of seats in the entering class, specialized high schools set a threshold score for admissions. Students also rank the schools that they wish to attend, so if they beat the cutoff score for their first-choice school, they are granted admission. If not, they drop to their second choice which may have a lower threshold score, and so on.

In other districts, the current admissions system eschews tests altogether, relying on a slew of other methods for evaluating academic performance and potential. For example, no standardized test scores are collected during the admissions process for any of the ASHSs in Washington, D.C.[7] Instead, each school relies on its own mix of grade cutoffs, essays,

[6]The New York City specialized high schools include: The Bronx High School of Science, The Brooklyn Latin School, Brooklyn Technical High School, Fiorello H. LaGuardia High School of Music & Art and Performing Arts, High School for Mathematics, Science and Engineering at City College of New York, High School of American Studies at Lehman College, Queens High School for the Sciences at York College, Staten Island Technical High School, and Stuyvesant High School.

[7]https://enrolldcps.dc.gov/sites/dcpsenrollment/files/page_content/attachments/English_SY22-23DCPSSelectiveHSAdmissionsInformationOverview.pdf

recommendation letters, and interviews to determine its admissions offers.

1.2 Admissions Policy Changes

For decades, ASHSs across the US have faced criticism for not being racially/ethnically nor economically diverse (we provide more representation details in Section 2.5). Lowell High School in San Francisco, California, provides a useful case study for the controversies and forces that drive ASHSs to alter their approach to admissions. Lowell became an ASHS in 1966 after it became oversubscribed, screening students based on their GPA. In 1983, the consent decree from San Francisco NAACP v. San Francisco Unified School District mandated that no single race or ethnic group could make up more than 40 percent of enrollment.[8] By 1995, this policy led to Chinese American students being held to higher standards on the admissions test than other groups.[9] In February 1996, the San Francisco Board of Education changed Lowell's policy to fill between 70-80 percent of admission slots based only on grades and test scores. The remaining 20-30 percent were then filled with consideration for socioeconomic status, extracurriculars, past challenges, and race.[10] In 2001, following the settlement of Ho v. San Francisco Unified School District which disallowed race-based admissions decisions[11], Lowell first implemented a three-level admissions system that closely resembles the current policy.[12] For the 2024-25 school year, Lowell's admissions system admits students in the same three bands.[13] First, 70 percent of seats are allocated to students based on middle school grades and score on the admissions test. Another 15 percent of seats are distributed to applicants who are evaluated based on a rubric that includes various indicators of economic and personal hardship, extracurricular pursuits, and service (students must still meet minimum grade and admissions test score requirements). The final 15 percent of seats are allocated to students who attend middle schools that are underrepresented at Lowell among the previous three Lowell cohorts (students must still meet a minimum score and must be nominated by their principal).

ASHSs' admissions policies are diverse and evolve over time. In this paper, we focus on a relatively recent subset of ASHSs admissions policy changes that are defined by increased targeting of SES diversity through a variety of mechanisms.

We focus on four types of SES-based admissions policies. First are Low-Income Quota admissions policies that prioritize students who are categorized as low-income (New York, NY).

[8] https://law.justia.com/cases/federal/district-courts/FSupp/576/34/2593333/
[9] https://www.proquest.com/docview/2021890024
[10] https://www.proquest.com/docview/2047718203
[11] https://clearinghouse.net/case/9799/
[12] https://go.boarddocs.com/ca/sfusd/Board.nsf/goto?open&id=AZS28M7BDEA9
[13] https://drive.google.com/file/d/1SGCkuFxJvcVIdaYJK8MWpvo1pLrfvyYT/view

Second are ELC policies, which allocate a minimum number of seats for the top-performing students from each middle school in the district (Alexandra, VA; Charleston, SC). Third are Priority Group policies that target students who meet certain special circumstances, such as low parental educational attainment, living in a low-income neighborhood, or attending a low-income middle school (Austin, TX; Charleston, SC; San Francisco, CA). And fourth is an SCA policy, referred to as the Discovery Program in New York City, which allows for an alternative route to admission for disadvantaged students in the city's specialized high schools through a summer training program. Appendix B provides a detailed description of each of these policies. The full set of observed policy changes is described in section 2.

2 Data

This study links three data sets to investigate the impacts of admissions policy changes at ASHSs. First, we use a database of ASHSs across the United States. Second, we create a database of ASHS admissions policy changes. Finally, we use school enrollment data to analyze the effects of policy changes.

2.1 Database of Academically Selective High Schools

In defining ASHSs and generating our list of them, we follow Finn and Hockett (2012). That is, we set six conditions that define an ASHS. Each school must be public, have a 12th grade, be a stand-alone school, teach a college-prep curriculum, select students on academic criteria, and have competitive admissions. Finn and Hockett (2012) identify 165 ASHSs in the United States. We augment this list with an additional 13 schools based on a search for historical lists of ASHSs and an assessment of current admissions policies for the top 150 high schools in the 2022 U.S. News and World Report rankings (2023).[14] We drop six schools from Finn and Hockett (2012) that are not found in the National Center for Education Statistics Common Core of Data, leaving us with a list of 172 ASHSs.

2.2 Admissions Policy Changes

We create a new panel data set of 122 admissions policy changes affecting 76 ASHSs between the late 2000s and early 2020s. We use official district and school websites to ascertain cur-

[14]Sunny Hills High School, Cass Technical High School, Davis Aerospace High School, Renaissance High School, Southwestern Classical Academy, International Studies Charter High School, Columbus High School, International Academy of Macomb, Middlesex County Voc Acad Allied Health & Biomedical, Science/ Math Science & Engineering Techn, Central Magnet School, Judge Barefoot Sanders Law Magnet, Young Women's Leadership Academy, and Academy for Information Technology/Allied Health Sciences

rent school admissions policies. From there, we check for archived historical policies using the Internet Archive Wayback Machine. When available, we rely on these official admissions materials. Policy changes are recorded when there are observable adjustments to the application requirements or rubric for evaluating applications. However, when admissions policy documents are not regularly and thoroughly archived, we search local news publications for mentions of the ASHS and its admissions policy.

Relying on these approaches, we categorize policy changes along several dimensions. In this study, we focus on SES-based policies that adjust admissions requirements to prioritize low-SES applicants. These policies can take several forms, including Low-Income Quotas; ELC policies where the top performing students from local middle schools are automatically admitted; Priority Groups from disadvantaged backgrounds; or SCA admissions, which accept students from low-income backgrounds who score below the admissions cutoff but are required to attend a summer bridge program. We observe 56 instances of admissions policy changes related to SES status. These are the changes that we focus on as treatments in our analysis. We denote schools as potential controls if they have no policy change of any type between 2005 and 2022.

2.3 School Demographics

We link our admissions policy data with the National Center for Education Statistics' Common Core of Data (CCD), a comprehensive database of public school enrollment by free- or reduced-price-lunch eligibility (FRPL), free-lunch eligibility (FL), and race/ethnicity (U.S. Department of Education, 2023).[15] The CCD provides race/ethnicity enrollment counts at the grade-by-school-by-year-level, which allows us to study changes at the 9th grade cohort-level. On the other hand, the FRPL and free-lunch enrollment counts data are only available at the school-by-year-level, limiting us to study socioeconomic compositional changes over time at the school-level.[16] We impute the average between the previous and following school-year information when a school has missing demographic enrollment information for a single year.

From this data, we restrict the sample for analysis to include only selective schools that are classified as traditional schools rather than alternative schools, vocational schools, or college-based programs. Based on data availability considerations described in section 2.5, we are left with 60 control units and up to 28 treated schools when we use an unbalanced

[15] As suggested by the NCES, we calculate free-lunch based on the non-missing maximum between free-lunch eligible and direct certification counts (Sinclair & Chen, 2020).

[16] Through various initiatives, some schools have instituted universal school lunch programs over time, however Section 4.4 shows that our main results are robust to excluding observations and/or schools that report having 100% free-lunch eligibility.

panel for analysis. This data allows us to calculate enrollment shares by socioeconomic status and race/ethnicity.

2.4 College Readiness

In order to study within-school changes across cohorts' college readiness, we compile a panel dataset of school-level average ACT and/or SAT scores. To do this, we collect publicly available data published by state departments of education or individual school districts. When historical datasets had not already been compiled, we relied on the Internet Archive Wayback Machine or FOIA requests to compile these data.

We map SAT scores into equivalent ACT scores by using published concordance tables.[17] We use the ACT scoring scale because it has used a consistent scoring system over time, while the SAT changed its scoring system in 2016. When both scores are available, we take the average between the published ACT score and the converted SAT score in order to use as much information as possible. We also interpolate data for schools missing single years of data by taking the average of the one-year lead and lag. From the school-by-year level data, we use ACT scores to estimate college readiness for the corresponding year's graduating cohort of seniors.

Since the scores are based on cohorts taking the test during their senior year, we require a four-year lag to observe outcomes. This cuts our analytic sample of ASHSs down by about 42%. Specifically, we only have college-readiness data for 25% and 73% of the treated and control schools from our final analytic sample, respectively. Therefore, we limit our college readiness analysis to the subsample for which we have both demographic and test data.

2.5 Descriptive Statistics

2.5.1 Demographic Representation at Selective and Non-Selective Schools

Taking a step back from our final analytic sample, our compiled dataset allows us to illustrate the socioeconomic and racial representation at ASHSs and non-ASHSs across the US over time. As shown in Figure 1, ASHSs are located in many parts of the US, but are heavily concentrated in some of the largest urban districts in the United States. Therefore, it is important to select the appropriate comparison group to compare the attributes of these schools and their students with their non-selective counterparts. We restrict our comparison

[17] For scores between 2005 and 2016 we use the concordance table found at: https://blog.prepscholar.com/act-to-sat-conversion. For scores starting in 2017 we use the concordance table found at: https://satsuite.collegeboard.org/media/pdf/guide-2018-act-sat-concordance.pdf

group of non-ASHSs to those that are located in a district that has at least one ASHS during the year of observation.

ASHSs underrepresent economically disadvantaged students (as proxied by free- or reduced-price lunch eligibility enrollment shares). Figure 2 shows the average low-income enrollment rates at ASHSs over time. The dark green shading represents the share of students who are free-lunch eligible, while the light-green shading represents the share eligible for reduced-price lunch. The two panels are based on a balanced sample of schools over time that serve 9th graders, for fair comparison.

Panel (a) shows that for the past decade, free-lunch (FL) students have, on average, made up less than half of ASHS students. On the other hand, panel (b) shows that, since 2012, FL students make up more than 60% of students at non-ASHSs. Over time, economically disadvantaged students have become a larger part of ASHS enrollment. Between 2010 and 2022, low-income student enrollment shares at ASHSs increased by 39% compared to about a 14% increase at non-ASHSs. This change took place almost entirely in the first half of the decade.

Asian and White students are overrepresented at ASHSs. Figure 3 compares ASHSs (panel a) to traditional, non-selective public schools (panel b) based on the racial composition of their 9th-grade cohorts between 2010 and 2022. These figures show that Asian and White students (orange hues) make up a larger enrollment share at ASHSs than at non-ASHSs. On the other hand, Black and Hispanic students (blue hues) are underrepresented at ASHSs relative to the non-ASHSs in the same districts.

URM representation at ASHSs has worsened over time. Between 2010 and 2015, there was a decline of 3.3% in URM enrollment shares at ASHSs, even though URM students made up an increasing share of 9th grades at non-ASHSs. This has slowly improved in the second part of the decade.[18]

On the other hand, Asian enrollment shares at ASHSs have increased by about 8%, while declining by 11% at non-ASHSs between 2010 and 2022. White enrollment shares have declined at both school types, but this change has been slower at ASHSs (-17%) than at non-ASHSs (-23%). Enrollment shares for other race groups have increased at a slightly slower pace in ASHSs than at non-ASHSs over time.[19]

[18]Through time, Black students have made up a decreasing share of ASHSs and non-ASHSs schools. Between 2010 and 2015, Black enrollment share declined by 9% at ASHSs, but only by 5% at non-ASHSs. During the second half of the decade, the share of Black students has declined more slowly at ASHSs than at non-ASHSs. On the other hand, Hispanic enrollment shares increased at similar rates before 2015, but since then have increased by 17% at ASHSs compared to 9% at non-ASHSs.

[19]Appendix figures A1 and A2 show very similar trends when using an unbalanced sample of schools, but still requiring that there is an ASHS in the district in the same year.

2.5.2 Analytic Sample of Exam Schools

When analyzing the impacts of newly implemented admissions policies on the composition of the entering cohorts at ASHSs, we do not compare their enrollment trends to those of non-ASHSs. Instead, we compare our treated ASHSs to ASHSs that did not implement changes to their admissions policies during our sample period. Table 1 compares our outcomes of interest across several relevant school groups during the 2015-16 school year—prior to any of the policy treatments that meet the criteria for inclusion in our analytic sample.[20]

Columns 1-3 characterize our unrestricted list of ASHSs. Column 1 describes the full sample of ASHSs that appear in the NCES Common Core of Data, have non-missing enrollment-by-grade-by-race data in 2015, are classified as traditional high schools,[21] and have a 9th grade class. Column 2 averages across schools in our sample that were never treated by an admissions policy change during our sample period (2010 through 2022). Column 3 restricts to those schools that were treated by an admissions policy change that targeted prospective students by socioeconomic status.[22] Untreated and SES treated schools have similar shares of white students, but SES treated schools have a higher proportion of Asian students—29% compared to 16%, on average—while untreated schools have a higher share of underrepresented minority (URM) students—51% compared to 34%.

Columns 4-6 of Table 1 describe the final sample that we use for analysis, which excludes observations with missing race or FL enrollment data. We also restrict the SES treated schools to require that there are at least six years without a policy change before the relevant policy change goes into effect. This leaves our final sample of 60 control and 28 SES treated ASHSs. We utilize an unbalanced panel in which schools are treated for anywhere between 1 and 4 periods depending on the existence of subsequent policy changes and data availability, due to policy recency—Table 3 lists the treated schools in our analysis sample.[23] Columns 5 and 6 of Table 1 show that entering classes at treated schools have relatively larger shares of Asian and White students, and smaller shares of FL or URM students than the control group.

Table 2 provides descriptive statistics for the subsample of schools for which we are able to compile college readiness data. Columns (5) and (6) show that our final analytic test scores subsample is made up of seven treated and 45 control ASHSs. Generally, the patterns

[20] We discuss our decision rules for simultaneous or consecutive policy changes in appendix C.

[21] Other possible classifications include special education, vocational, alternative, and reportable programs. Schools are dropped from the sample for any year in which they are not classified as a traditional school.

[22] Twenty-four schools from Column 1 are excluded from Columns 2 or 3 because they had admissions policy changes during the sample period that were not directly related to applicants' SES.

[23] While most of our treated ASHSs are from NYC, in robustness checks, we show that our policy impact estimates are consistent with or without treated New York City ASHSs.

follow those of the main sample. Additionally, the treated schools have higher ACT scores than the control schools in the analytic subsample for which we have test data.

3 Empirical Strategy

We estimate the effects of implementing class-based affirmative action on the demographic composition of academically selective high schools. Our approach compares changes in the differences in enrollment shares between ASHSs that implement SES-based affirmative action admissions policy and ASHSs that continue with their status quo admissions during the sample period. To identify these effects, we leverage geographic and temporal variation in a staggered difference-in-difference (DID) framework.

More specifically, we estimate regressions of the following form:

$$Y_{sdt} = \alpha + \beta * AdmissionsChange_{st} * Post + \theta_t + \mu_s + \epsilon_{sdt} \tag{1}$$

where Y_{sdt} represents the enrollment share at school s in district d in year t for a specific demographic group (FL eligible, White, Asian, URM, Black, or Latino). The key variable of interest, $AdmissionsChange_{st}$, is a treatment measure denoting the fraction of the unit of observation that will experience an admissions policy change in the corresponding relative year. Our race/ethnicity outcome data is available at the grade-by-school-by-year level, allowing us to capture immediate policy impacts on each incoming cohort of 9th graders ($AdmissionsChange_{st} = 1$ for treated schools when analyzing race-based outcomes). On the other hand, our FL-eligibility data is only available at the school-by-year level. Therefore, in the year of the policy shock, only one out of four cohorts in a high school is treated ($AdmissionsChange_{st} = 0.25$) and by the fourth year, the full school-year-level observation is treated ($AdmissionsChange_{st} = 1$).[24] We set $AdmissionsChange_{st}$ to 1 for all treated units during the pre-shock periods. Our control group of untreated schools is "uncontaminated" in two main ways. First, in the sense that it only includes ASHSs that did not experience any policy change in our sample period ($AdmissionsChange_{st} = 0$ for every year in the sample).[25] Second, it excludes non-ASHSs that may have experienced spillover-effects,

[24] In robustness section D, we further discuss the implications and benefits of using a dose-response approach in our setting.

[25] We allow ASHSs that had multiple admissions policy changes, as long as they meet our requirement that the treatment timing does not overlap with a previous or future admissions policy change at the same school. That is, we require that the policy change must be at least six years after any previous policy change and at least one year prior to any future policy change. In practice, no school appears more than once in our analysis sample, though many schools have multiple policy changes.

where a reform could have pushed students from ASHSs into non-ASHSs, and vice-versa.[26]

This model includes year fixed effects, θ_t, to control for any time-varying changes in school demographics that are experienced across schools. We also include school fixed effects, μ_s, which control for time-invariant quality differences between schools. The school fixed effects account for any underlying school characteristics, such as prestige or desirability, that are known by school applicants before a policy change. The inclusion of both year and school fixed effects means the estimates are identified from within-school changes in demographics after an admissions policy change.

The coefficient of interest, β, represents the DID estimate of the impact of a full-dose policy change on the demographic composition of interest after implementation, relative to the composition prior to the policy change. Note that the model excludes $AdmissionsChange_{st}$ without the post indicator because, like other time-invariant school characteristics, this is absorbed by the school fixed effects. Standard errors are clustered at the district-level to account for similarity of policies and treatment timing across ASHSs within a district.

In the context of this study, policy implementation may not be random. For example, ASHSs may decide to change their admission policies because of stagnation or deterioration in their diversity, or because of new social pressures within the district. Appendix Figure A3 plots enrollment share outcomes by event time, covering six pre-shock years and four post-shock years. Control ASHSs are displayed in black and each treatment timing group is shown in a different color.[27] Consistent with the decade-long national trends described in Section 2.5, the control group shows increasing FL enrollment shares and consistently stable URM enrollment shares across our time period. The solid lines represent linear trend estimates based on pre-shock data, which vary across treatment timing groups. The figure clearly shows that different treatment timing groups exhibit different enrollment shares patterns in the pre-shock period.

To ensure we avoid potential bias in our estimates due to school-level variation in pre-implementation trends, we follow the literature and directly control for pre-trends by applying a two-step de-trending method (East et al., 2023; Goodman-Bacon, 2021). Specifically, we estimate linear outcome pre-trends for each school using only pre-policy data, which we extrapolate for each school through the post-shock period. We then subtract the fitted values from the observed values for each outcome, which provides outcome estimates for each relative year, accounting for any differential pre-trend. This does not change the estimated trend breaks; it only modifies the orientation of the event-study coefficients (Goodman-

[26] In robustness tests, we also exclude control ASHSs within the same district as treated ASHSs that may have experienced spillover effects.

[27] We center event time at 2016 to be the reference year for control units because this is the first admissions policy change in our sample, which is standard in the literature (East et al., 2023; Wolfers, 2006).

Bacon, 2021).

Two main assumptions underlie our DID approach. First, any differential pre-trends must be approximately linear to be accounted for by our specification. This assumption is analogous to parallel pre-trends in the traditional DID specification. We test this assumption by conducting event study analyses that explore the yearly policy change effect on the outcomes of interest over time, after removing school-specific linear pre-trends. Therefore, these event studies provide information on whether there are any remaining nonlinear differences across ASHSs in the pre-period.

We estimate a standard event study using regressions of the following form:

$$Y_{sdt} = \lambda + \sum_{\tau=-6}^{3} \Big(\gamma_\tau * AdmissionsChange_{st} * 1(RelativeYear = \tau) \Big) + \theta_t + \mu_s + \epsilon_{sdt} \quad (2)$$

where the γ_τ coefficients represent the change in the relationship between an eventually/fully treated unit of observation and the outcome of interest in relative period τ.

Recent econometric studies have highlighted potential issues with the two-way fixed effects (TWFE) model and its corresponding event studies (Roth et al., 2023). Sun and Abraham (2021) highlight that the traditional TWFE event study yields consistent results only if certain stringent assumptions about homogeneous treatment effects across treated groups and/or across time are met. Therefore, to address concerns about the robustness of our main results and to allow for heterogeneity in treatment effects across time and treated units, we present event studies that employ robust estimators introduced by Callaway and Sant'Anna (2021), Sun and Abraham (2021), Wooldridge (2021), and de Chaisemartin and D'Haultfœuille (2020). These approaches deliver consistent estimates even in the presence of heterogeneous treatment effects across time and/or treated units, by shutting down DID comparisons between newly treated and already treated units.

The DID approach also assumes that there are no contemporaneous unobservable shocks that are correlated with the treatment and the outcome of interest. Although this assumption is not directly testable, our catalog of ASHS admissions policy changes allows us to exclude schools with multiple simultaneous admissions adjustments from our analysis. For example, our sample of treated schools excludes all Chicago ASHSs because the policy change that took place in 2009 replaced a race-based AA policy with an SES place-based policy. While we would be interested in the impacts of the SES place-based policy in Chicago, the termination of the race-based admissions policy would contemporaneously impact admissions in a way

that would obscure the impact of the SES-based policy.[28]

4 Results

We begin our empirical analysis of the effects of ASHS admissions policy changes by examining event studies and DID models, as described in section 3. We first evaluate the impacts on low-income enrollment shares (based on FL eligibility rates) and then on the racial/ethnic composition of ASHSs. We then explore policy-type heterogeneity and compare our results to predictions from the previous literature.

4.1 Effects on Enrollment Shares, by Free-Lunch Status

Figure 4 presents an event study based on equation 2, which estimates the dynamic relationship between the activation of an SES-based AA admissions policy and FL enrollment shares at ASHSs from six years before a policy change and up to three years after implementation. Each dot represents the estimated coefficient, and the bands represent the 95% confidence interval. The dashed vertical line separates the pre- and post-policy shock periods. Since the outcome of interest is a measure between 0 and 1, each γ_τ represents the percentage point difference in the relationship between the eventual or implemented admissions policy change and the outcome of interest in period τ, relative to the most recent pre-shock year ($\tau = -1$).

The event study shows that after accounting for differences in school-specific linear pre-trends the estimates of the relationship between relative time and FL enrollment shares are close to zero and relatively flat during the pre-shock period. Thus, the figure provides supporting evidence for the linear pre-trends assumption, suggesting negligible remaining non-linear differences across treatment and control ASHSs in the pre-period. The implementation of an SES-based AA admissions policy change in period 0 is associated with a visually clear and significant increase in FL enrollment shares. The abrupt increase makes it clear that our estimates represent policy effects rather than the continuation of a pre-existing trend.[29]

Table 4 presents analogous DID estimates based on equation 1. Panel A shows that controlling for school-specific pre-trends, there is an average increase of about 14.4 percentage points (pp) in FL enrollment shares in response to a full dose of treatment, which is statis-

[28] We discuss cases and our decision rules for simultaneous/consecutive policy changes in appendix C.

[29] As a robustness check, in section 4.4.2, we show that event studies based on robust estimators that allow for heterogeneity in treatment effects across time and treated units have very similar dynamics to those estimated by our main approach.

tically significant at the 1% level. These results are for an unbalanced sample that includes all control schools and treated schools with one to four post-shock observations.

In Panel B, we limit the analysis to all pre-shock periods and only include the first post-shock period, which ensures a balanced sample. Using this more limited analysis sample, we find a slightly larger impact of about 18.4pp, which is still statistically significant at the 1% level. Taking these estimates at face value, they suggest a 40% to 51% increase in FL enrollment shares. These findings show that the admissions policy changes are effective at increasing the representation of the lowest-income students.

4.2 Effects on Enrollment Shares, by Race/Ethnicity

The evidence above strongly suggests that the implementation of SES-based affirmative action policies had the first-order effect of improving the representation of low-income students, as proxied by FL eligibility. While these policies were intended to be race-neutral, there may be differential impacts by race/ethnicity due to the widely documented strong correlation between family income and race/ethnicity (Chetty et al., 2020).

Panel (a) of Figure 5 presents event study estimates for 9th grade cohorts' URM enrollment shares before and after SES-based admission policy implementation, defined as the sum of Black and Hispanic enrollment shares. Accounting for differences in linear pre-trends, there are no remaining pre-shock differences between treatment and control states. In treated ASHSs, the period following the policy change exhibits a clear break in trend, with an immediate and statistically significant increase in URM enrollment shares within the first entering 9th grade cohort that is admitted under the new admissions policy.

Panels (b) through (d) in Figure 5 present event studies of the estimates for enrollment shares in 9th grade cohorts over time, by non-URM groups. Panel (b) shows that the increase in URM enrollment shares, discussed above, is almost entirely driven by an abrupt and significant decline in White enrollment shares. Importantly, panel (c) shows no significant change in Asian enrollment shares. However, the other race enrollment shares see a slight decline during the second post-shock period (shown in panel (d)).

Table 5 summarizes the corresponding DID results based on equation 1. Panel A shows the results for the unbalanced sample across the four post-shock periods, while panel B limits the estimated effects to the first post-shock period for a balanced sample. Column (1) shows that the SES-based admissions policies lead to a 7.9pp increase in URM enrollment shares, which is statistically significant at the 1% level—an 18% increase relative to the mean. On the other hand, there is a 5.3pp decline in White enrollment shares (17%), which is statistically significant at the 5% level. Column (3) shows that, on average, there is insufficient evidence

to reject the null hypothesis that ES-based admissions policies have a no impact on Asian enrollment shares. The URM and White coefficients do not exactly counteract each other because there is a slight decline in other race enrollment shares of 2.6pp and a decline of 1.1pp in other race enrollment shares, which is only statistically significant at the 10% level. Panel B shows the effects are qualitatively similar for the balanced sample.

4.3 Heterogeneity

In this section, we explore heterogeneity by specific type of SES-based admissions policy that is implemented. To do this, we categorize our SES-based admissions policy changes into four groups: first, Low-Income Quotas are a type of policy that requires a certain fraction of admitted/enrolled students to be low-income; second, eligibility in the local context (ELC) policies give admissions priority to the top performing students of feeder middle schools (sometimes referred to as a Top-Percent policy); third, Priority Group-based policies give admissions priority to students from targeted groups based on a specific or combination of characteristics (e.g. having attended a low-income middle school, residence in a certain income-level neighborhood, parental educational attainment, identification as URM at the ASHS, participation in extracurriculars, etc.); and fourth, submarginal conditional acceptance (SCA) is an admissions policy that provides conditional admissions to students from low-income backgrounds that scored marginally below the admissions exam cutoff and who attend a summer training program.[30]

To explore differential effects by policy type, we interact $AdmissionsChange_{st}$ with indicators for each of the four specific SES admissions policies in equation 1. This provides an estimated enrollment effect for each policy type.

The estimated effects across policy types are qualitatively similar. Figure 6 shows that all four SES-based AA admissions policies have a positive and statistically significant impact on FL enrollment shares. More specifically, we estimate that Low-Income Quotas has the largest impact, followed by ELC, Priority Groups, and, lastly, by SCA. Simulations by Cestau et al. (2017) and Ellison and Pathak (2021) predict that these SES-based admissions policies would be effective at increasing the representation of low-SES students. The difference in effectiveness is in line with predictions by Ellison and Pathak (2021), who argue that more direct measures of SES, such as FRPL, are more effective at increasing low-SES representation than indirectly prioritizing low-income students based on their neighborhood-SES through ELC or Priority Group policies. However, our estimates suggest that the coefficients for Low-Income Quotas, ELC, and Priority Groups are not significantly different from each

[30] In appendix B, we provide more details on each of the SES-based admissions policies that were implemented by ASHSs in our analytic sample.

other.

We estimate that the four policy types increased FL enrollment shares by 26% to 65%, which is similar compared to a 39% to 82% increase predicted in ELC and FL-priority simulation studies (Cestau et al., 2017; Corcoran et al., 2018; Rucinski & Goodman, 2022). However, this is in contrast to substantially smaller policy impacts estimated at the university level, where Kapor (2020) estimates a 5.4% effect from the Texas' ELC admissions policy on enrollment of students from high-poverty schools and Bleemer (2023) finds a negligible effect from California's ELC policy on low-SES student enrollment.

In terms of the effects on race/ethnicity, panel (a) of figure 7 show that all four admissions policies had statistically significant positive effects on the URM enrollment shares at ASHSs, ranging from increases of 14% to 27%. These estimates are smaller compared to those estimated through theoretical and simulation-based predictions of similar policies in academically selective K-12 schools, which estimate increases of more than 50% in URM representation from FL priority or ELC policies (Cestau et al., 2017; Corcoran et al., 2018; Rucinski & Goodman, 2022). However, our estimates of the effects on URM enrollment are larger than those found in the college context (Bleemer, 2023; Kapor, 2020).

The Priority Group policy had the largest impact on URM enrollment shares, with an increase of 13pp, or 27%. For the Low-Income Quotas and the Priority Group policies, the URM effects are driven by significant declines in White enrollment shares of about 10pp, or -32%. The largest policy impact on Asian enrollment shares was from the ELC policy, with a decline of 8pp (36%), while the most positive impact on Asian enrollment shares was from the Low-Income Quotas, with an increase of 2.3pp, or 11%. Lastly, the SCA's effect on URM enrollment shares is not driven by changes in White/Asian students, but instead more so by a 5.2pp decline in other race student enrollment shares.

4.4 Robustness

4.4.1 Dose-Response vs. Traditional DID and Event Study Estimates

We now test the sensitivity of our main findings by conducting a series of robustness checks. First, our results are robust to not incorporating a dose-response to our DID approach. As described in section 2, our socioeconomic enrollment data is only available at the school-by-year-level. Our main empirical approach incorporates dose-response to address the fact that our socioeconomic unit of observation is only partially impacted in the first three years that the policy is implemented. With proper scaling, this approach is mathematically equivalent to the traditional framework without dosage. Furthermore, our preferred approach provides estimates that are more easily interpretable and comparable than the traditional

DID approach, given the limitations of the socioeconomic enrollment data.[31] We provide more details in Appendix D.

4.4.2 Robust Estimators for Staggered DID

Second, our estimates are robust to bias from heterogeneous treatment effects across groups or time. Sun and Abraham (2021) highlight that the traditional TWFE event study yields consistent results only if certain stringent assumptions about homogeneous treatment effects are met. To allow for heterogeneity in treatment effects across time and treated units, we present event studies that employ robust estimators introduced by Callaway and Sant'Anna (2021), Sun and Abraham (2021), Wooldridge (2021), and de Chaisemartin and D'Haultfœuille (2020).

Figure A4 shows that for all our outcomes of interest, the pre-trends and post-shock effects are consistent across estimators. The robust estimators cannot accommodate dose-response DID, therefore, the event study for FL enrollment shares in panel (a) is estimated in the traditional way, without dosage. As described in Appendix D, FL event studies that do not incorporate dosage will differ from our dose-response estimates in periods +0, +1, and +2, however, these are equivalent with proper rescaling.

4.4.3 Robustness to concerns about the sample

A third potential concern is that our estimated effects are driven by only New York City.[32] To test this, we replicate our analysis on two separate samples. The first sample excludes all treated New York ASHSs, while the second sample includes only treated ASHSs from New York, with the same set of control schools used in both samples. Appendix table A1 presents quantitatively and quantitatively similar estimates of the impact of implementing SES-based AA on the low-income and racial composition at non-NYC ASHSs (in panel A) and exclusively at NYC ASHSs (in panel B). However, due to the smaller sample size, the estimates are noisier and have larger standard errors.

The only coefficient that changes direction is that of the impact on Asian enrollment shares, which is negative when focusing on non-NYC treated ASHSs and positive when limiting to NYC treated ASHSs. However, the standard errors on these estimates are larger than the coefficients in both cases–still failing to reject the null hypothesis. This test suggests that the impacts we find are broadly representative of ASHSs, and not driven solely by NYC

[31] The dose-response DID approach estimates coefficients that are interpreted in the same way for both socioeconomic outcomes (school-by-year-level data) and for race/ethnicity outcomes (grade-by-school-by-year-level data).

[32] As shown in table 3, the majority of treated ASHSs in our analytic sample are in New York City.

ASHSs.

Next, Appendix Table A2 shows the results from various specifications for the effects of implementing an SES-based AA admissions policy. Column (1) shows the main results for comparison. To address the concern that perhaps our main results are driven by pandemic-era admissions policies, columns (2) and (3) show that we find similar estimates when we limit to pre-pandemic treated units and drop all observations before 2020 or before 2021. Columns (4) and (5) show that we estimate similar coefficients when we control for year-to-year district-level demographic changes, providing evidence that we are estimating school-specific effects rather than district-wide demographic changes.

Another potential concern is that in districts with multiple treated ASHSs, admissions policy changes in the most in-demand ASHS may have trickle-down effects for ASHSs that are less in-demand. Thus, lower-ranked ASHSs may not be able to attract as many AA-targeted students, whether or not they also instituted an SES-based AA admissions policy. To address these concerns, we keep only the ASHS with the highest average ACT scores if there are multiple ASHSs in the district in column (6),[33]. Additionally, in column (7) we drop all control ASHSs in districts with treated ASHSs from the sample. In both subsamples, we estimate similar, although slightly larger, coefficients.

Furthermore, through various initiatives some schools, districts, and states have instituted universal school lunch programs over time, potentially impacting some of the schools in our sample. In columns (8) and (9) we still estimate similar coefficients, even when we exclude all observations that report 100% FL eligibility or ASHSs that ever report having a 100% FL eligibility.

4.4.4 Secular Trends and Reversion to the Mean

Another potential concern is that our estimated policy impacts reflect secular trends or reversion to the mean that would have happened even without policy implementation. To investigate this, we conduct placebo tests where we randomly assign treated ASHSs to implementation years and test whether the new estimated effects are as large as our baseline policy impact estimates. In panel A of appendix table A3, we distribute the treated ASHSs equally across our implementation years between 2016 and 2022. In panel B, we randomly distribute the treated ASHSs to match our timing distribution, mimicking the post-shock data availability balance from our actual sample.

We randomize placebo treatment dates 100 times for each outcome and calculate the percent of cases that generated policy impact estimates larger than our baseline estimates. Therefore, these results represent p-values of the null hypothesis that any combination of

[33]Effectively, this drops non-Stuyvesant ASHSs in NYC and non-Boston Latin ASHSs in Boston.

policy implementation dates would generate as extreme of policy impact estimates as our results. In table A3, we reject the null of such a hypothesis for almost all estimates. However, we cannot reject the null of such a hypothesis for Asian enrollment shares. This makes sense because our main estimate for Asian enrollment shares is not statistically significant.

These results suggest our main estimates are not identified based on secular trends. Although reversion to the mean may play some part in enrollment share changes, these placebo tests show that our main estimates are strongly linked to whether an SES-based admissions policy is implemented and to the specific timing with which the policy is implemented. These findings support the validity of our estimation strategy. Furthermore, if reversion to the mean is playing a role in our estimates, this further suggests that we are estimating a lower-bound of the actual policy impact.

5 Impacts on college readiness

Next, we analyze the impacts of SES-based AA admissions policies on within-school changes in average cohort-specific college readiness. To do this, we focus on a subsample of ASHSs for which we have ACT and/or SAT data, as described in Section 2. Since the scores are based on cohorts taking the test during their senior year, we require a four-year lag to observe outcomes with the correct timing. This cuts our sample of ASHSs down by about 42%. Specifically, we only have college-readiness data for 25% and 73% of the treated and control schools from our final analytic sample, respectively.

Columns (1) through (5) of Table 6 show that the test scores sample exhibits very similar compositional impacts to those from the main analytic sample, with an increase of 14pp (38%) in FL shares and a 15.5pp (3%) increase in URM enrollment shares, both of which are statistically significant at the 1% level. Asian enrollment shares have a positive coefficient of 1.3pp, but this is not statistically significant. Again, those gains in enrollment shares come from declines in White and other race enrollment shares. We present the corresponding event studies in Figure A5.

Figure 8 shows that the gains in FL and URM enrollment shares, do not have a significant impact on college readiness for the corresponding cohorts at treated ASHSs. Column (6) of Table 6 shows that the average ACT score for the treated cohorts actually increased by 0.29 points, or 1.2%, although this is not statistically significant. Using the 95% confidence interval, our estimate rules out an increase in average ACT scores of greater than 1.08 points and rules out declines greater than 0.5 points.

Research based on simulations suggests that SES-based AA admissions policies increase FL and URM enrollment shares, but at the cost of lower entering average achievement, espe-

cially for URM students (Corcoran & Baker-Smith, 2018; Ellison & Pathak, 2021; Rokkanen, 2015). However, Rokkanen (2015) shows that these types of policies can improve performance on high school standardized tests overall, especially for the students whose assignment is altered by the reform. Our findings provide suggestive evidence supporting those hypotheses.

6 Conclusion

Selective educational institutions provide students with access to competitive learning environments, substantial resources, and high-achieving peers (Chetty et al., 2023; Dobbie & Fryer, 2014; Finn & Hockett, 2012). For decades, these institutions have struggled to diversify their campuses and expand access to high-quality education for students from different social backgrounds. While universities have been at the center of this debate, the topic is equally controversial for admissions to highly-coveted K–12 academically selective schools.

In this paper, we first provide a descriptive analysis of the SES and racial/ethnic diversity at public, academically selective high schools across the United States. We then provide the first empirical evidence on the effects of socioeconomic class-based admissions policies on the demographic composition at these schools. To do this, we create a novel national dataset of admissions policies at academically selective high schools (ASHSs) and changes in these policies over time, which we link to national 9th-grade cohort and high school-level demographic data.

We show that selective schools are concentrated in large cities that serve a significant fraction of the US student population. We find that, on average, low-income, Black, and Latino students are underrepresented at ASHSs compared to non-ASHSs in districts with at least one ASHS. This under-representation has been shrinking for low-income students, but growing for URM students over time. We then use geographic and temporal variation to show that SES-based affirmative action admissions policies lead to increases in low-income and URM enrollment shares. These effects are almost entirely driven by declines in White enrollment shares, with very little, if any, changes in Asian enrollment shares.

Importantly, we show that SES-based admissions policies that directly target low-SES students through Low-Income Quotas, ELC, and Priority Groups have the largest and most statistically significant impacts on free-lunch enrollment shares, while SCA has smaller effects. In terms of race/ethnicity, all four admissions policies that we investigate have positive and statistically significant effects on the URM enrollment shares at ASHSs.

Lastly, we find suggestive evidence that, on average, SES-based admissions policies lead to null, or slightly positive, impacts on college readiness, even though they admit students with lower past performance levels. Our findings support predictions that have been proposed by

relevant theoretical and simulation-based research.

Data limitations and the recency of these policy changes, impede us from studying the impacts on advanced course enrollment, bullying, and behavioral issues. We leave the exploration of these important and policy-relevant impacts to future work.

This paper fills a gap in the economics and education literature and provides insights for policymakers and administrators at selective educational institutions into the potential effects of SES-based affirmative action admissions policies, which do not explicitly rely on race—making them permissible under the latest Supreme Court rulings against race-based affirmative action.

References

Abdulkadiroglu, A., Angrist, J. D., & Pathak, P. A. (2014). The Elite Illusion: Achievement Effects at Boston and New York Exam Schools. *Econometrica*, *82*(1), 137–196. https://doi.org/10.3982/ECTA10266

Angrist, J. D., Pathak, P. A., & Zarate, R. A. (2023). Choice and consequence: Assessing mismatch at Chicago exam schools. *Journal of Public Economics*, *223*, 104892. https://doi.org/10.1016/j.jpubeco.2023.104892

Barrow, L., Sartain, L., & de la Torre, M. (2020). Increasing Access to Selective High Schools through Place-Based Affirmative Action: Unintended Consequences. *American Economic Journal: Applied Economics*, *12*(4), 29.

Black, S. E., Cortes, K. E., & Lincove, J. A. (2015). Academic Undermatching of High-Achieving Minority Students: Evidence from Race-Neutral and Holistic Admissions Policies. *American Economic Review*, *105*(5), 604–610. https://doi.org/10.1257/aer.p20151114

Black, S. E., Denning, J. T., & Rothstein, J. (2023). Winners and Losers? The Effect of Gaining and Losing Access to Selective Colleges on Education and Labor Market Outcomes. *American Economic Journal: Applied Economics*, *15*(1), 26–67. https://doi.org/10.1257/app.20200137

Bleemer, Z. (2023). Affirmative Action and its Race-Neutral Alternatives. *Journal of Public Economics*. https://doi.org/https://doi.org/10.1016/j.jpubeco.2023.104839

Callaway, B., & Sant'Anna, P. H. C. (2021). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, *225*(2), 200–230. https://doi.org/10.1016/j.jeconom.2020.12.001

Cestau, D., Epple, D., & Sieg, H. (2017). Admitting students to selective education programs: Merit, profiling, and affirmative action. *Journal of Political Economy*, *125*(3), 761–797. Retrieved October 15, 2017, from http://www.journals.uchicago.edu/doi/abs/10.1086/691702

Chetty, R., Deming, D. J., & Friedman, J. N. (2023). Diversifying Society's Leaders? The Determinants and Causal Effects of Admission to Highly Selective Private Colleges. *NBER Working Paper*. https://doi.org/10.3386/w31492

Chetty, R., Hendren, N., Jones, M. R., & Porter, S. R. (2020). Race and Economic Opportunity in the United States: An Intergenerational Perspective. *The Quarterly Journal of Economics*, *135*(2), 711–783. https://doi.org/10.1093/qje/qjz042

Corcoran, S. P., Jennings, J. L., Cohodes, S. R., & Sattin-Bajaj, C. (2018). Leveling the Playing Field for High School Choice: Results from a Field Experiment of Informational Interventions. *NBER Working Paper*. Retrieved September 15, 2022, from

https://www-nber-org.proxy.library.cornell.edu/system/files/working_papers/w24471/w24471.pdf

Corcoran, S. P., & Baker-Smith, E. C. (2018). Pathways to an Elite Education: Application, Admission, and Matriculation to New York City's Specialized High Schools. *Education Finance and Policy*, *13*(2), 256–279. https://doi.org/10.1162/edfp_a_00220

Cortes, K. E. (2010). Do bans on affirmative action hurt minority students? Evidence from the Texas Top 10% Plan. *Economics of Education Review*, *29*(6), 1110–1124. https://doi.org/10.1016/j.econedurev.2010.06.004

de Chaisemartin, C., & D'Haultfœuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, *110*(9), 2964–2996. https://doi.org/10.1257/aer.20181169

Dobbie, W., & Fryer, R. G. (2014). The Impact of Attending a School with High-Achieving Peers: Evidence from the New York City Exam Schools. *American Economic Journal: Applied Economics*, *6*(3), 58–75. https://doi.org/10.1257/app.6.3.58

East, C. N., Miller, S., Page, M., & Wherry, L. R. (2023). Multigenerational Impacts of Childhood Access to the Safety Net: Early Life Exposure to Medicaid and the Next Generation's Health. *American Economic Review*, *113*(1), 98–135. https://doi.org/10.1257/aer.20210937

Ellison, G., & Pathak, P. A. (2021). The Efficiency of Race-Neutral Alternatives to Race-Based Affirmative Action: Evidence from Chicago's Exam Schools. *American Economic Review*, *111*(3), 943–975. https://doi.org/10.1257/aer.20161290

Feerick Center for Social Justice. (2019). *Screened Out: The Lack of Access to NYC Screened Program Admissions Criteria* (tech. rep.). Fordham Law School. Retrieved August 27, 2024, from https://www.fordham.edu/media/home/schools/school-of-law/pdfs/screened-out-accessible.pdf

Finn, C. E., & Hockett, J. A. (2012, September). *Exam Schools*. Princeton University Press. Retrieved July 8, 2023, from https://press.princeton.edu/books/hardcover/9780691156675/exam-schools

Goodman-Bacon, A. (2021). The Long-Run Effects of Childhood Insurance Coverage: Medicaid Implementation, Adult Health, and Labor Market Outcomes. *American Economic Review*, *111*(8), 2550–2593. https://doi.org/10.1257/aer.20171671

Hoxby, C. M. (2003). School choice and school competition: Evidence from the United States. *Swedish Economic Policy Review*.

Kapor, A. (2020). Distributional Effects of Race-Blind Affirmative Action [Number: 2020-51 Publisher: Princeton University. Economics Department.]. *Working Papers*. Retrieved January 16, 2024, from https://ideas.repec.org//p/pri/econom/2020-51.html

Klasik, D., & Cortes, K. E. (2022). Uniform admissions, unequal access: Did the top 10% plan increase access to selective flagship institutions? *Economics of Education Review*, *87*, 102199. https://doi.org/10.1016/j.econedurev.2021.102199

Mader, N., Kramer, A., & Butel, A. (2018, November). *Promising Outcomes, Limited Potential: Diversity in Admissions in New York City Public Schools* (tech. rep.). Center for New York City Affairs. New York, NY.

Perez, J. (2018). Competition remains fierce for top programs as CPS sends out high school offers. *Chicago Tribune*. Retrieved August 27, 2024, from https://www.chicagotribune.com/2018/03/30/competition-remains-fierce-for-top-programs-as-cps-sends-out-high-school-offers/

Rokkanen, M. (2015). *Extrapolation and bandwidth choice in the regression discontinuity design* [Doctoral dissertation, Massachusetts Institute of Technology]. Retrieved October 15, 2017, from https://dspace.mit.edu/handle/1721.1/90131

Roth, J., Sant'Anna, P. H. C., Bilinski, A., & Poe, J. (2023). What's trending in difference-in-differences? A synthesis of the recent econometrics literature. *Journal of Econometrics*. https://doi.org/10.1016/j.jeconom.2023.03.008

Rucinski, M., & Goodman, J. (2022). Racial Diversity and Measuring Merit: Evidence from Boston's Exam School Admissions. *Education Finance and Policy*, *17*(3), 408–431. https://doi.org/10.1162/edfp_a_00343

Sinclair, B., & Chen, C.-S. (2020, August). Understanding School Lunch Eligibility in the Common Core of Data. https://nces.ed.gov/blogs/nces/post/understanding-school-lunch-eligibility-in-the-common-core-of-data

Stuyvesant High School. (2023). Admissions. Retrieved January 22, 2024, from https://stuy.enschool.org/m/pages/index.jsp?uREC_ID=126615&type=d

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199. https://doi.org/10.1016/j.jeconom.2020.09.006

U.S. Department of Education. (2023). Common Core of Data [Education Data Portal (Version 0.20.0), Urban Insitute Urban Institute, made available under the ODC Attribution License]. https://educationdata.urban.org/documentation/

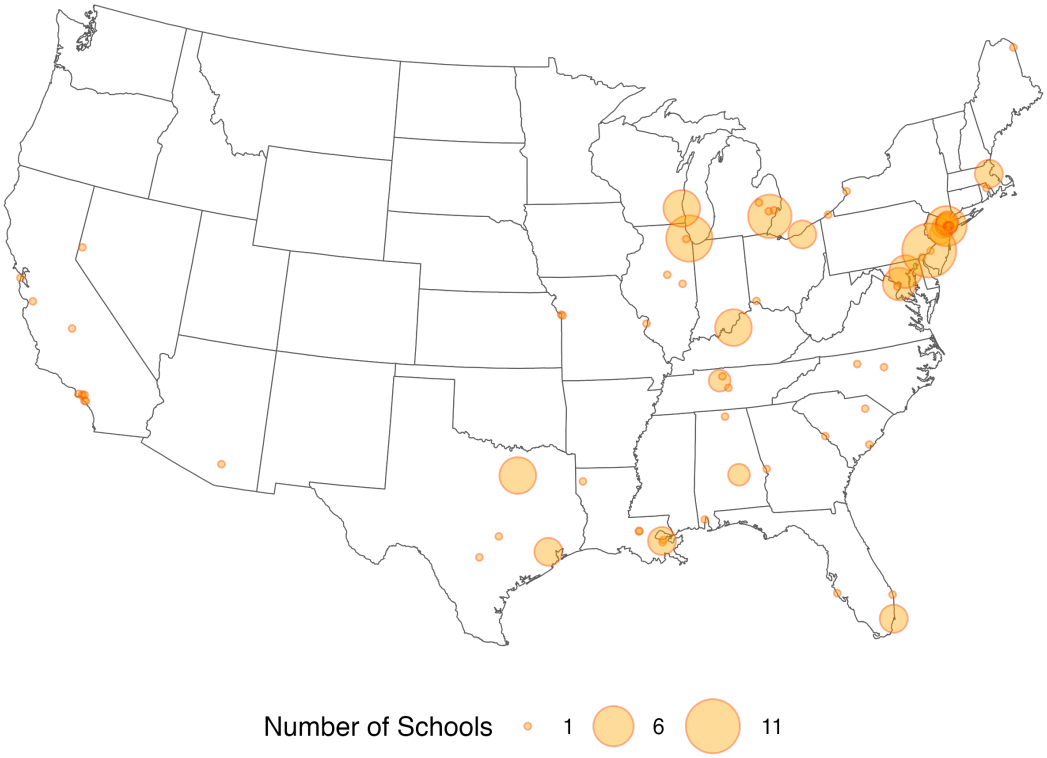
U.S. News and World Report. (2023). Best U.S. High Schools (2022-2023). https://www.usnews.com/education/best-high-schools/national-rankings

Wolfers, J. (2006). Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results. *The American Economic Review*, *96*(5), 19.

Wooldridge, J. M. (2021, August). Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators. https://doi.org/10.2139/ssrn.3906345

Figures

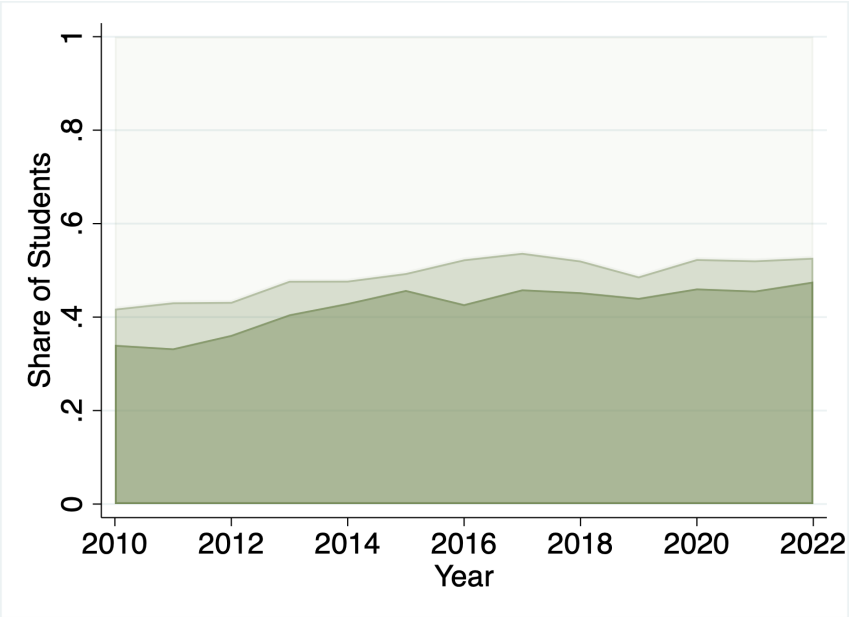
Figure 1: Map of school districts with at least one academically selective high school (ASHS)



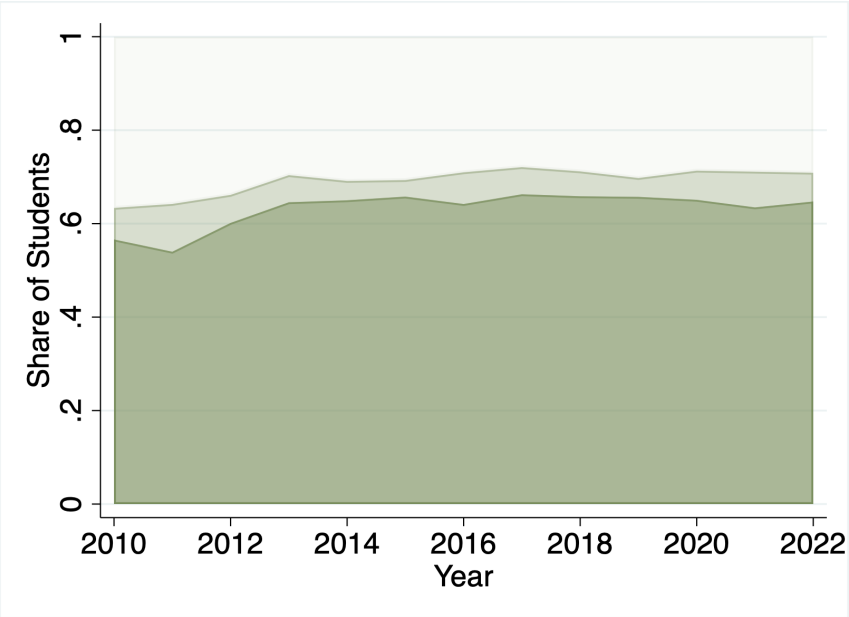
Notes: Each circle represents a separate school district. The circle size represents the number of 9th grade serving ASHSs in the corresponding school district between 2005 and 2021.

Figure 2: FRPL Enrollment Shares in ASHSs and Non-ASHSs

(a) Academically Selective High Schools



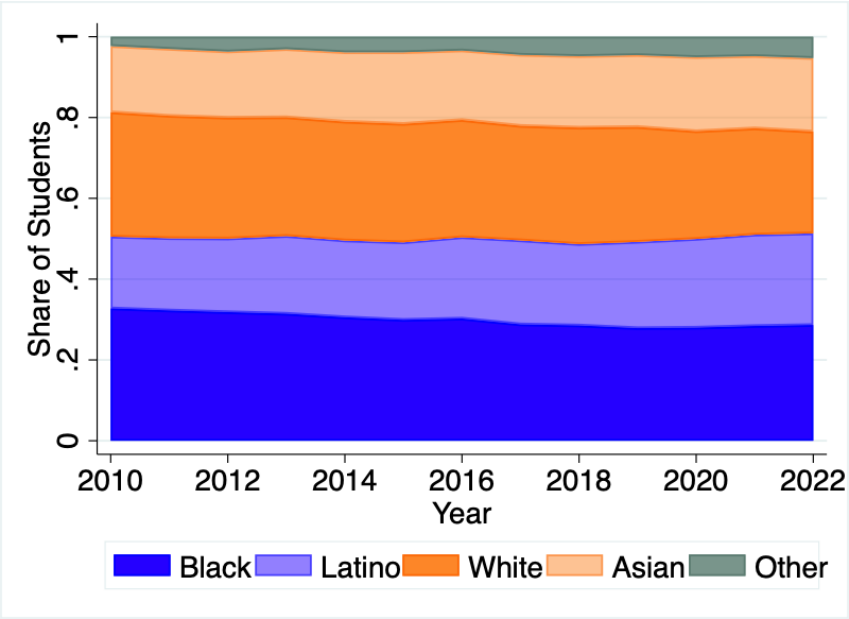
(b) Non-Academically Selective High Schools



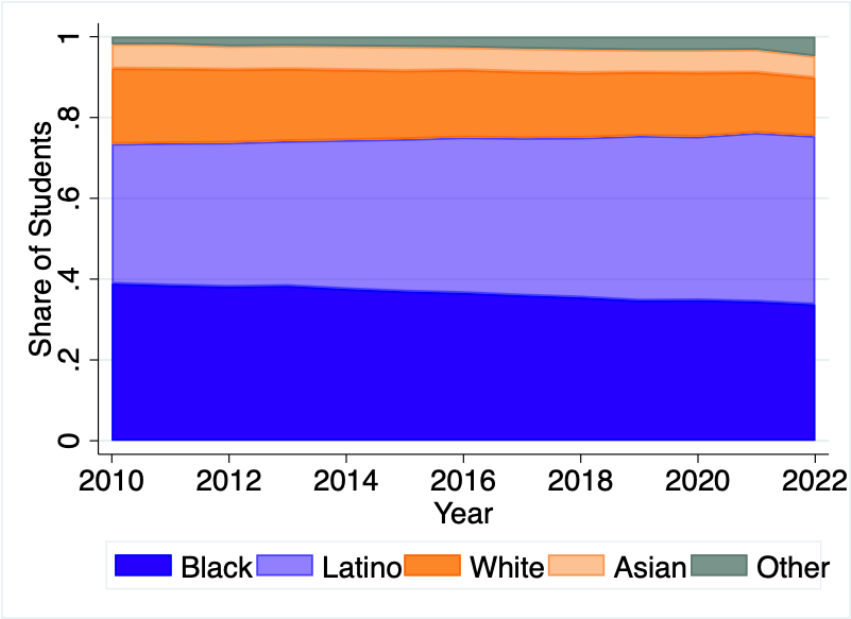
Notes: Enrollment shares by free-lunch and reduced-price-lunch status in ASHSs and non-ASHSs in districts that have at least one ASHS in the year of observation. The darkest, medium, and lightest green areas represent the free-lunch, reduced-price lunch, and non-FRPL enrollment shares, respectively. For comparability, this is a balanced sample of schools over time. Due to lack of by-grade free- or reduced-price lunch enrollment data, these shares are at the school-level (as long as there is a 9th grade in the school).

Figure 3: Share of 9th grade students in ASHSs and non-ASHSs, by race

(a) Academically Selective High Schools

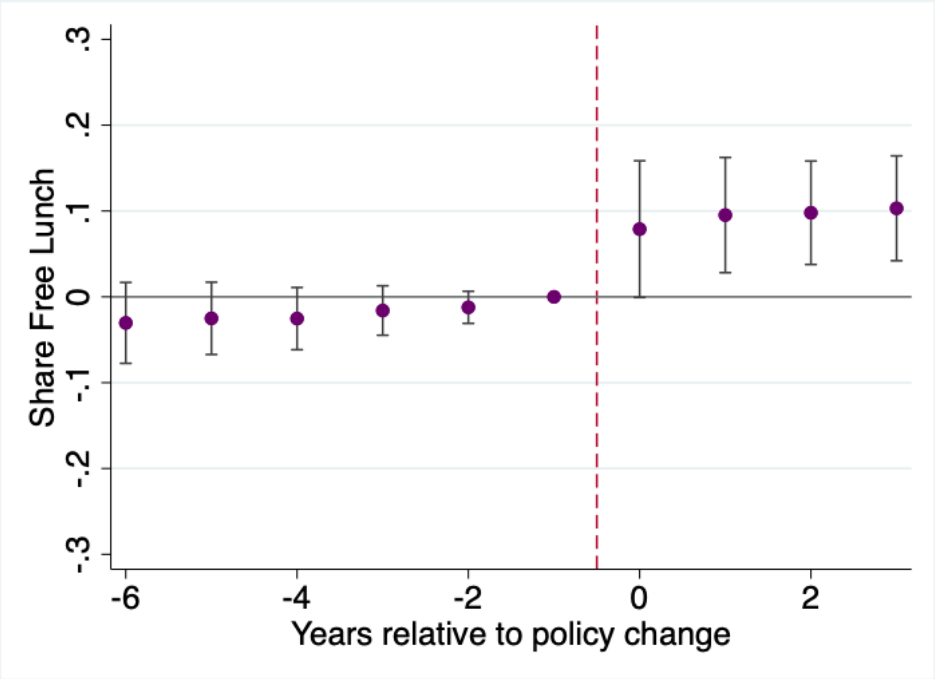


(b) Non-Academically Selective High Schools



Notes: The sample of non-ASHSs includes only schools in districts that have at least one ASHS in the year of observation. For comparability, this is a balanced sample of schools over time.

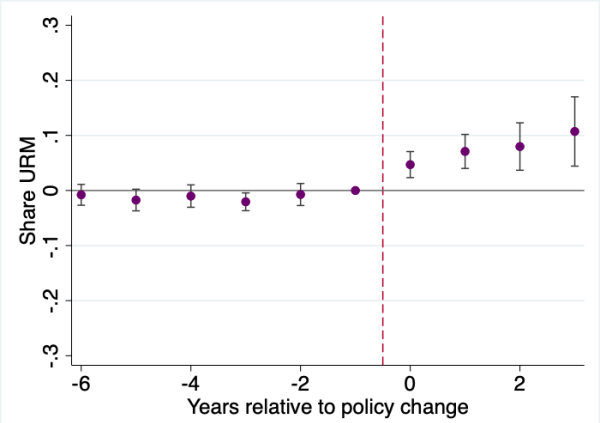
Figure 4: Effects on Free Lunch Enrollment Shares



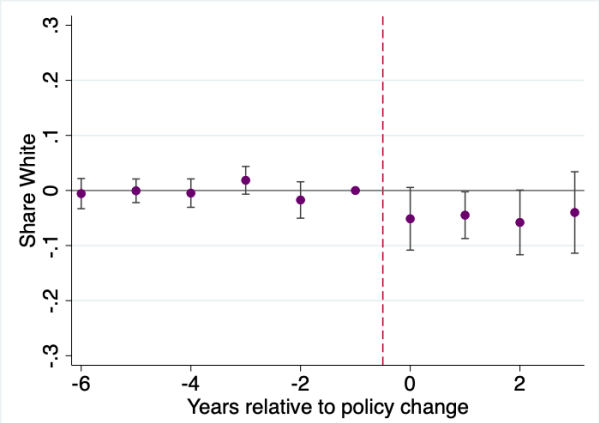
Notes: Event study is based on equation 2. Pre-period trend is estimated and removed from all observations for each school prior to the event study estimation, as described in section 3. For treated schools, this is estimated using all available pre-shock years for each school. For control schools, we use the 2010 to 2015 time-span to estimate the trend.

Figure 5: Effects on Racial/Ethnic Composition

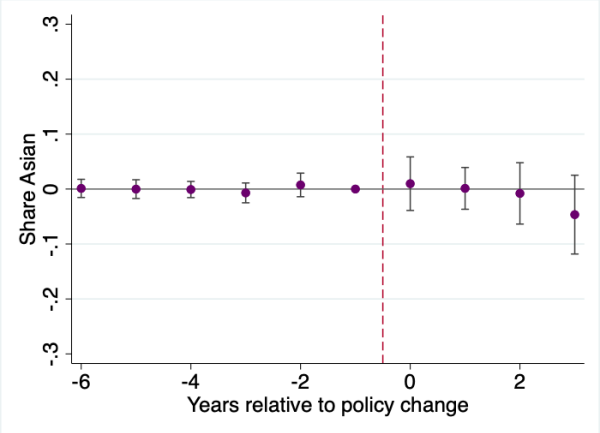
(a) URM Enrollment Shares



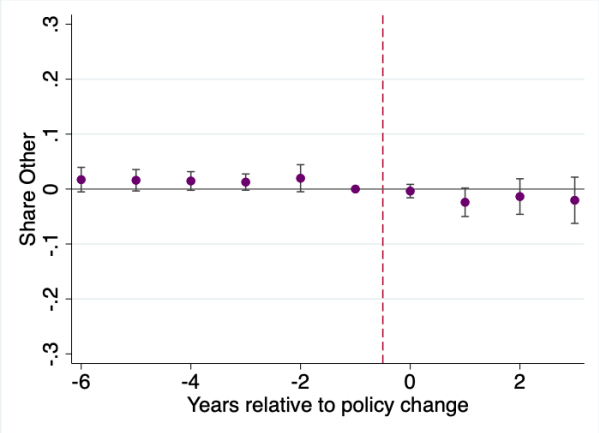
(b) White Enrollment Shares



(c) Asian Enrollment Shares

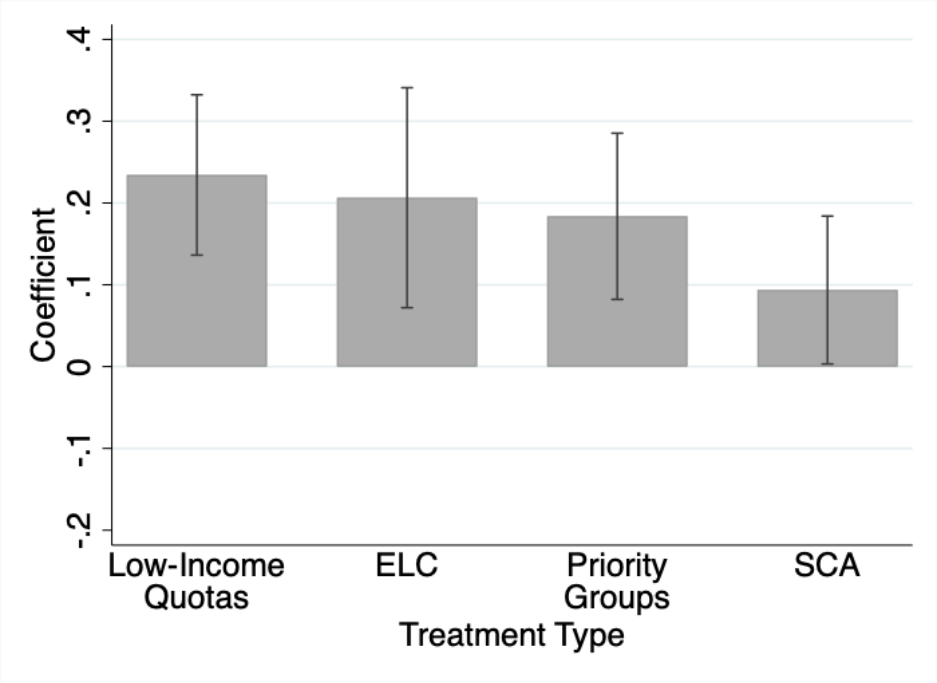


(d) Other Race Enrollment Shares



Notes: Event studies are based on equation 2. Pre-period trend is estimated and removed from all observations for each school prior to the event study estimation, as described in section 3. For treated schools, this is estimated using all available pre-shock years for each school. For control schools, we use the 2010 to 2015 time-span to estimate the trend. Each event study is based on a separate regression estimation.

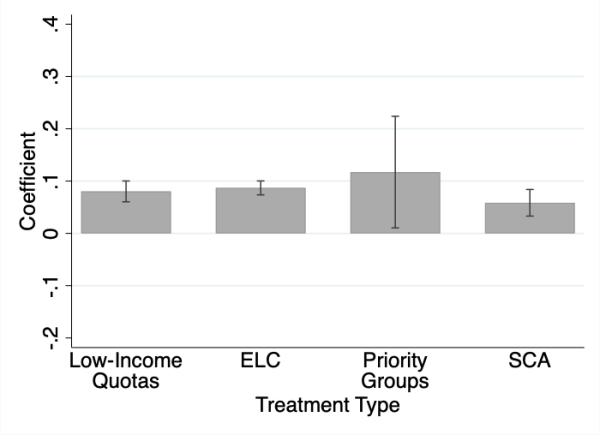
Figure 6: Effects on Free Lunch Enrollment Shares, by SES Treatment Policy Type



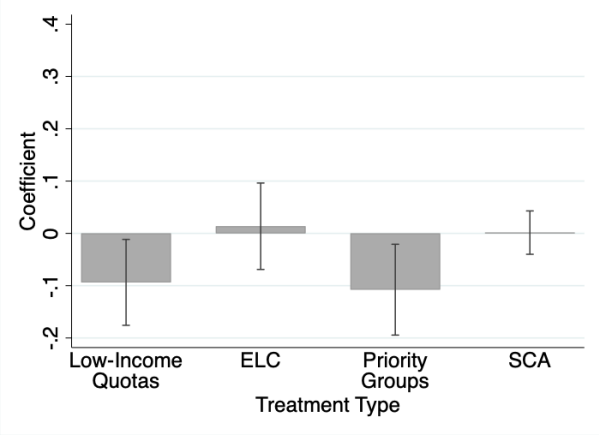
Notes: Bars represent DID estimates that adjust for school-specific pre-trends, as described in Section 3. The main coefficients are interacted with each of the four specific SES admissions policies to estimate separate policy type effects. Therefore, within each panel, all bars across the four policy types are based on the same regression estimation.

Figure 7: Effects on Racial/Ethnic Composition, by SES Treatment Policy Type

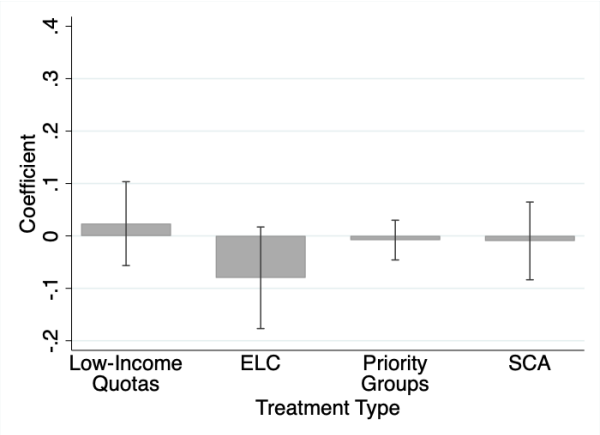
(a) URM



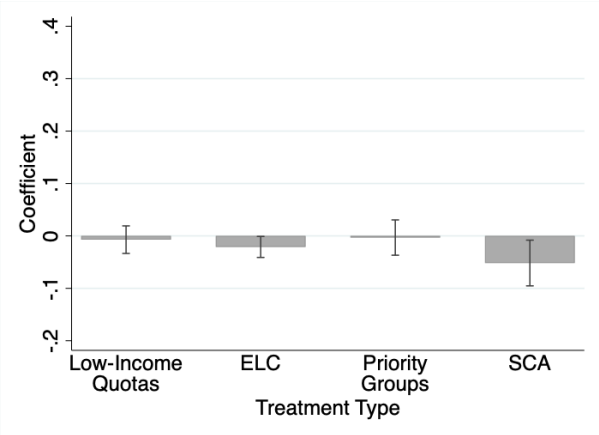
(b) White



(c) Asian

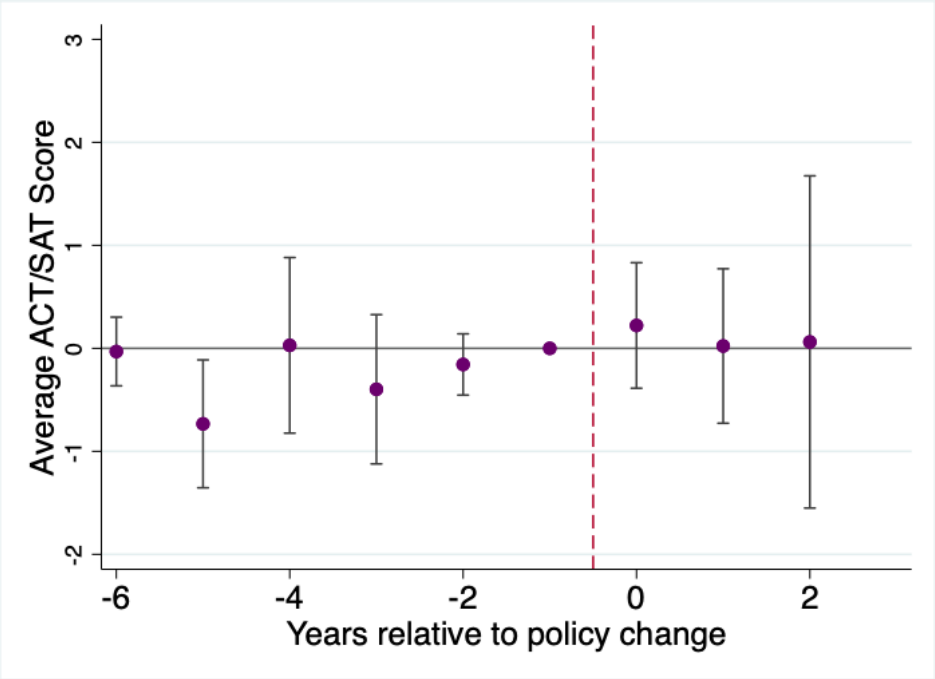


(d) Other Race



Notes: Bars represent DID estimates that adjust for school-specific pre-trends, as described in Section 3. The main coefficients are interacted with each of the four specific SES admissions policies to estimate separate policy type effects. Therefore, within each panel, all bars across the four policy types are based on the same regression estimation.

Figure 8: Effects on Average College Readiness



Notes: Event study is based on equation 2. Pre-period trend is estimated and removed from all observations for each school prior to the event study estimation, as described in section 3. For treated schools, this is estimated using all available pre-shock years for each school. We use the 2010 to 2015 time-span for control schools to estimate the trend.

Tables

Table 1: School Demographic Statistics — 2015–16 School Year

	Full Sample			Final Analysis Sample		
	ASHSs	Untreated	SES Treated	ASHSs	Control	SES Treated
Average Enrollment Shares by Economic Disadvantage						
Free Lunch	0.45	0.38	0.45	0.36	0.38	0.32
	(0.32)	(0.29)	(0.28)	(0.26)	(0.29)	(0.16)
Average Enrollment Shares by Race/Ethnicity						
Asian	0.19	0.16	0.30	0.22	0.16	0.36
	(0.21)	(0.21)	(0.21)	(0.23)	(0.21)	(0.21)
Black	0.29	0.29	0.18	0.22	0.29	0.08
	(0.30)	(0.28)	(0.24)	(0.26)	(0.29)	(0.08)
Hispanic	0.19	0.23	0.16	0.20	0.23	0.15
	(0.19)	(0.23)	(0.12)	(0.21)	(0.24)	(0.13)
URM	0.48	0.51	0.34	0.43	0.52	0.23
	(0.32)	(0.30)	(0.28)	(0.30)	(0.30)	(0.19)
White	0.30	0.30	0.31	0.31	0.29	0.36
	(0.23)	(0.24)	(0.20)	(0.22)	(0.23)	(0.20)
Other Race	0.04	0.03	0.05	0.04	0.03	0.05
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Average Cohort/School Size						
9th Graders	243.45	226.32	315.05	249.56	225.55	301.00
	(210.65)	(166.83)	(297.23)	(221.06)	(165.95)	(305.38)
Total Students	1025.72	949.13	1329.85	1057.39	933.75	1322.32
	(811.34)	(633.48)	(1104.45)	(851.51)	(623.42)	(1174.86)
N	130	62	40	88	60	28

Note: Summary statistics are based on sample described in section 2.5. Standard deviations are shown in parentheses. In the full ASHSs sample, Central Magnet School, Hume - Fogg High, Martin Luther King Jr School, Merrol Hyde Magnet School, and The Davidson Academy of Nevada are missing Free Lunch data in 2015. This means that the Share Free Lunch in column 1 is based on 125 observations and the Share Free Lunch in column 2 is based on 61 observations.

Table 2: School Demographic Statistics — 2015–16 School Year, Test Score Sample

	Full Sample			Final Analysis Sample		
	ASHSs	Untreated	SES Treated	ASHSs	Control	SES Treated
Average Enrollment Shares by Economic Disadvantage						
Free Lunch	0.50	0.43	0.46	0.40	0.43	0.25
	(0.31)	(0.29)	(0.27)	(0.28)	(0.29)	(0.17)
Average Enrollment Shares by Race/Ethnicity						
Asian	0.18	0.13	0.28	0.17	0.13	0.40
	(0.19)	(0.18)	(0.20)	(0.22)	(0.19)	(0.25)
Black	0.30	0.29	0.19	0.26	0.30	0.04
	(0.29)	(0.25)	(0.25)	(0.25)	(0.25)	(0.04)
Hispanic	0.20	0.24	0.16	0.22	0.24	0.08
	(0.20)	(0.25)	(0.12)	(0.24)	(0.26)	(0.05)
URM	0.50	0.53	0.35	0.48	0.54	0.12
	(0.32)	(0.30)	(0.28)	(0.32)	(0.30)	(0.07)
White	0.29	0.32	0.31	0.32	0.31	0.43
	(0.23)	(0.24)	(0.21)	(0.24)	(0.24)	(0.25)
Other Race	0.04	0.02	0.05	0.03	0.02	0.05
	(0.03)	(0.02)	(0.04)	(0.02)	(0.02)	(0.04)
Average Cohort/School Size						
9th Graders	246.59	232.02	302.50	271.87	231.24	533.00
	(212.94)	(162.44)	(299.14)	(252.28)	(161.06)	(508.75)
Total Students	1034.35	974.60	1281.21	1105.60	955.22	2072.29
	(800.31)	(605.75)	(1107.75)	(945.44)	(591.10)	(1943.11)
College Readiness						
Average ACT Score	24.63	24.07	26.86	24.83	23.87	31.01
	(5.20)	(5.03)	(4.15)	(5.30)	(5.03)	(1.48)
N	108	47	38	52	45	7

Note: Summary statistics are based on sample described in section 2.5. Standard deviations are shown in parentheses.

Table 3: Academically Selective High Schools with an SES-based Admissions Policy Change in our Analysis Sample

Year	School Name	School District	State	Treated Periods
2019	Brooklyn Latin School	New York City Geographic District #14	NY	4
2019	High School Mathematics, Science, and Engineering at CCNY	New York City Geographic District #5	NY	4
2019	Manhattan/Hunter Science High School	New York City Geographic District #3	NY	4
2019	Queens High School for the Sciences at York College	New York City Geographic District #28	NY	4
2019	Staten Island Technical High School	New York City Geographic District #31	NY	4
2018	Bard High School Early College Queens	New York City Geographic District #24	NY	4
2017	Liberal Arts and Sciences Academy (LASA) High School	Austin Independent School District	TX	4
2016	Academic Magnet High	Charleston County School District	SC	4
2020	Leon M Goldstein High School for the Sciences	New York City Geographic District #22	NY	3
2016	Brooklyn Technical High School	New York City Geographic District #13	NY	3
2016	High School of American Studies at Lehman College	New York City Geographic District #10	NY	3
2021	Beacon High School	New York City Geographic District #3	NY	2
2021	Thomas Jefferson High	Fairfax County Public Schools	VA	2
2019	Lowell High	San Francisco Unified	CA	2
2019	Millennium High School	New York City Geographic District #2	NY	2
2017	Bronx High School of Science	New York City Geographic District #10	NY	2
2022	Boston Latin	Boston Public Schools	MA	1
2022	Boston Latin Academy	Boston Public Schools	MA	1
2022	O'Bryant School Math/Science	Boston Public Schools	MA	1
2022	Townsend Harris High School	New York City Geographic District #25	NY	1
2021	Eleanor Roosevelt High School	New York City Geographic District #2	NY	1
2021	School of the Future High School	New York City Geographic District #2	NY	1
2020	Baruch College Campus High School	New York City Geographic District #2	NY	1
2020	NYC Lab High School for Collaborative Studies	New York City Geographic District #2	NY	1
2018	Stuyvesant High School	New York City Geographic District #2	NY	1
2023	New Explorations into Science, Tech, and Math High School	New York City Geographic District #1	NY	0
2023	Young Women's Leadership School	New York City Geographic District #4	NY	0
2023	Baccalaureate School for Global Education	New York City Geographic District #30	NY	0

Table 4: Effects of SES-based Admissions Policy on Free Lunch Enrollment Shares

	Share Free Lunch
	(1)
Panel A: Unbalanced Sample	
SES Policy Change X Post	0.144***
	(0.048)
Percent Effect	40.0%
Observations	978
Panel B: Balanced Sample (only 1 post-shock period)	
SES Policy Change X Post	0.184***
	(0.067)
Percent Effect	51.1%
Observations	838

Notes: Estimation is based on equation 1. Percent effect shows the effect relative to the mean of all ASHSs in the final analytic sample from table 1. Standard errors clustered at the school district level are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$

Table 5: Effects of SES-based Admissions Policy on Enrollment Shares, by Race/Ethnicity

	Share URM	Share White	Share Asian	Share Other
	(1)	(2)	(3)	(4)
Panel A: Unbalanced Sample				
SES Policy Change X Post	0.079***	-0.053**	0.000	-0.026*
	(0.016)	(0.024)	(0.021)	(0.014)
Percent Effect	18.4%	-17.1%	0.0%	-65.0%
Observations	978	978	978	978
Panel B: Balanced Sample (only 1 post-shock period)				
SES Policy Change X Post	0.056***	-0.049	0.010	-0.018
	(0.014)	(0.029)	(0.024)	(0.011)
Percent Effect	13.0%	-15.8%	4.5%	-45.0%
Observations	838	838	838	838

Notes: Estimation is based on equation 1. Percent effect shows the effect relative to the mean of all ASHSs in the final analytic sample from table 1. Standard errors clustered at the school district level are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$

Table 6: Effects of SES-based Admissions Policy on Enrollment Shares and College Readiness – Test Scores Sample

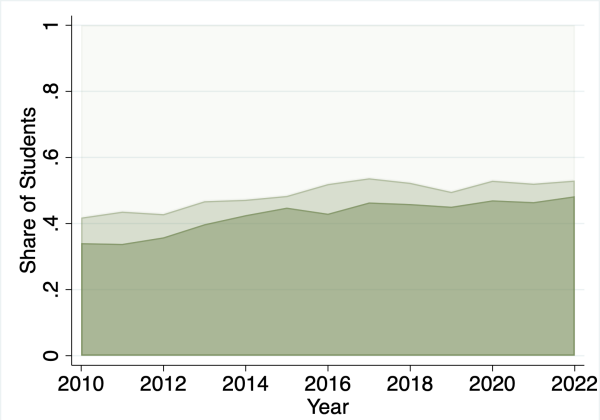
	Share Free Lunch	Share URM	Share White	Share Asian	Share Other	Avg. Coll. Readiness
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unbalanced Sample						
SES Policy Change X Post	0.136***	0.055***	-0.042*	0.013	-0.027	0.288
	(0.034)	(0.019)	(0.022)	(0.012)	(0.019)	(0.404)
Percent Effect	37.8%	12.8%	-13.5%	5.9%	-67.5%	1.2%
Observations	448	448	448	448	448	448

Notes: Estimation is based on equation 1. Percent effect shows the effect relative to the mean of all ASHSs in the final analytic sample. Average college readiness is based on school average ACT scores as described in section 2. The ACT score average of all ASHSs in the final analytic sample was 24.92 for 2015–16. Standard errors clustered at the school district level are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$

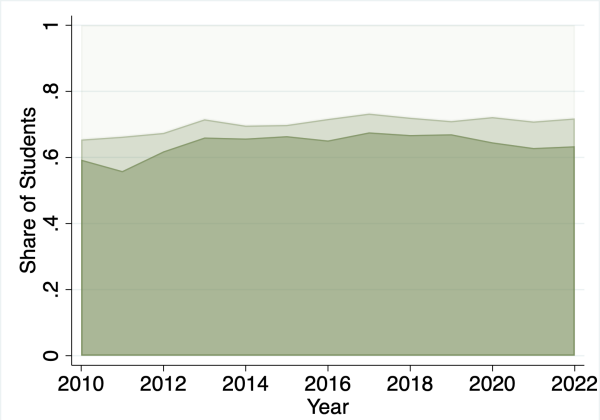
Appendix A Figures and Tables

Figure A1: FRPL Enrollment Shares in ASHSs and Non-ASHSs (unbalanced sample)

(a) Academically Selective High Schools



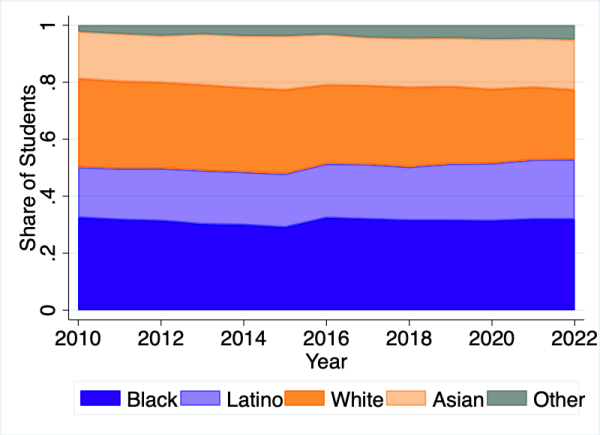
(b) Non-Academically Selective High Schools



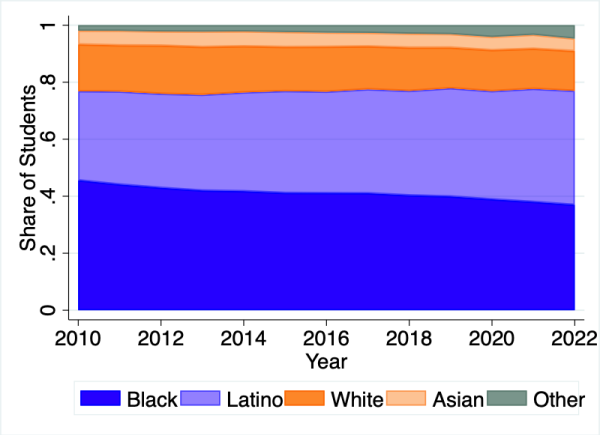
Notes: Enrollment shares by FRPL status in ASHSs and non-ASHSs in districts that have at least one ASHS in the same year. The darkest, medium, and lightest green areas represent the free-lunch, reduced-price lunch, and non-FRPL enrollment shares, respectively. For comparability, this is a balanced sample of schools over time. Due to lack of by-grade free- or reduced-price lunch enrollment data, these shares are at the school-level (as long as there is a 9th grade in the school). The schools in these figures are unbalanced, which means the schools and districts making up the averages can change over time.

Figure A2: Share of 9th grade students in ASHSs and non-ASHSs, by race (Unbalanced sample of schools)

(a) Academically Selective High Schools



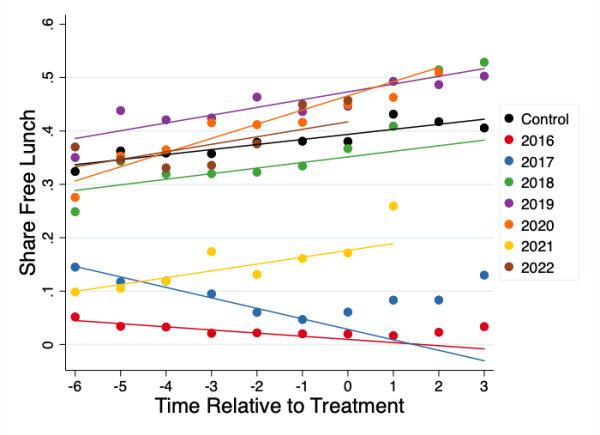
(b) Non-Academically Selective High Schools



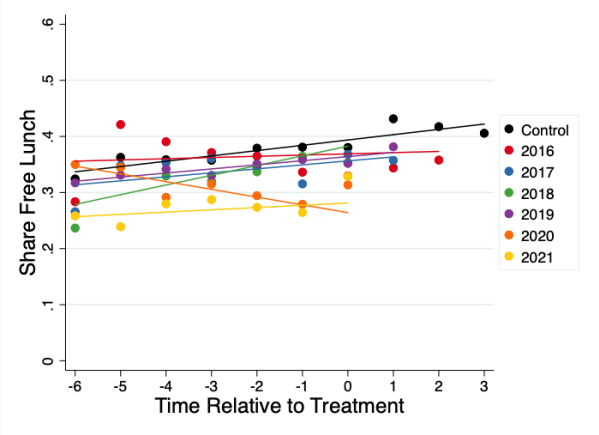
Notes: The sample of non-ASHSs includes only schools in districts that have at least one ASHS in the year of observation. The schools in these figures are unbalanced, which means the schools and districts making up the averages can change over time.

Figure A3: Trends in Demographic Enrollment Shares by Treatment Timing Group

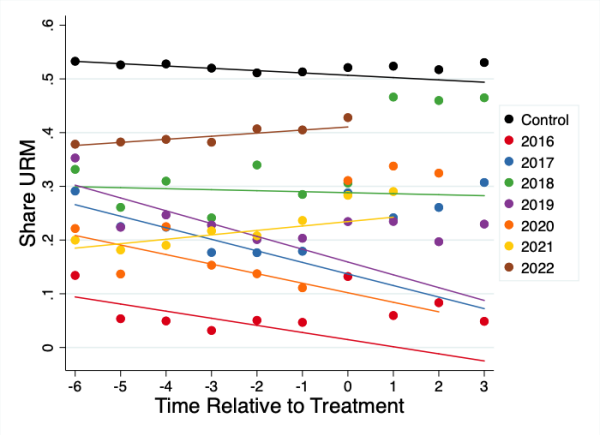
(a) Free Lunch, Single Change



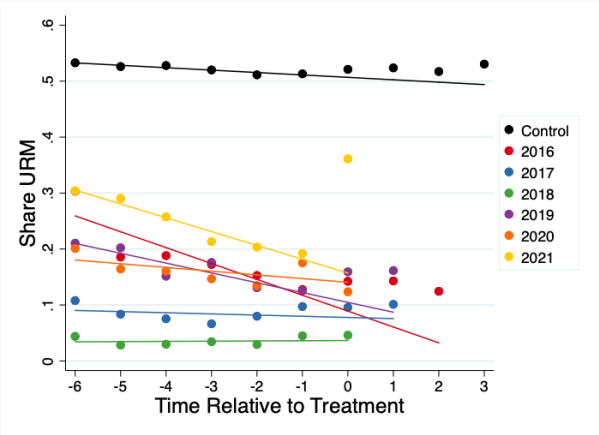
(b) Free Lunch, Cut short



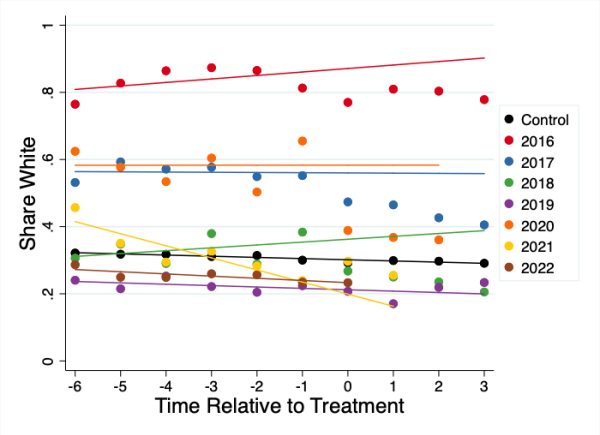
(c) URM, Single Change



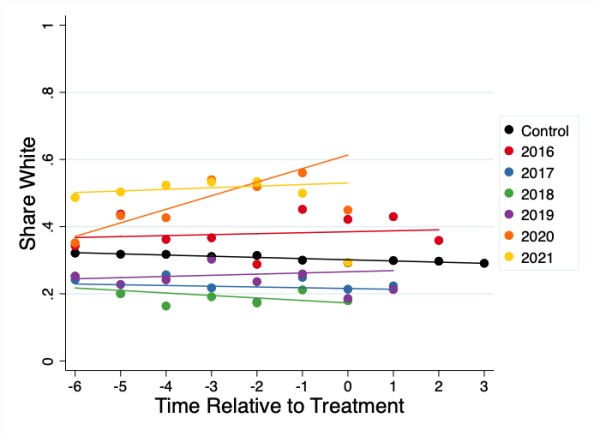
(d) URM, Cut short



(e) White, Single Change



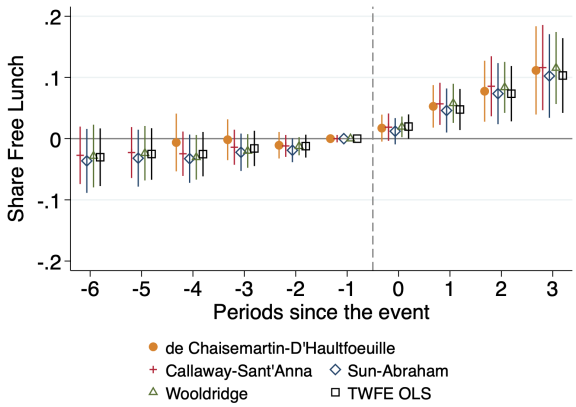
(f) White, Cut short



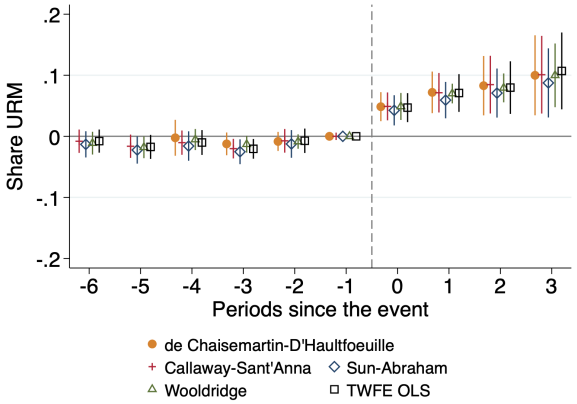
Notes: This figure displays raw enrollment shares by implementation timing group. Left-panels only include data for schools that are not cut short due to a follow-up admissions policy change. Right-panels only include data for schools that are cut short due to a follow-up admissions policy change.

Figure A4: Event Study Estimates – Using various estimators (without dose-response)

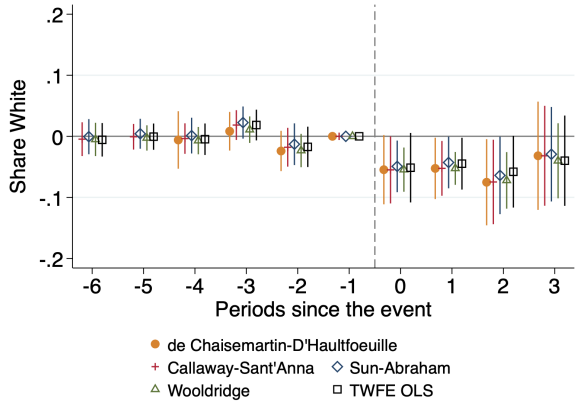
(a) Free Lunch Enrollment Shares



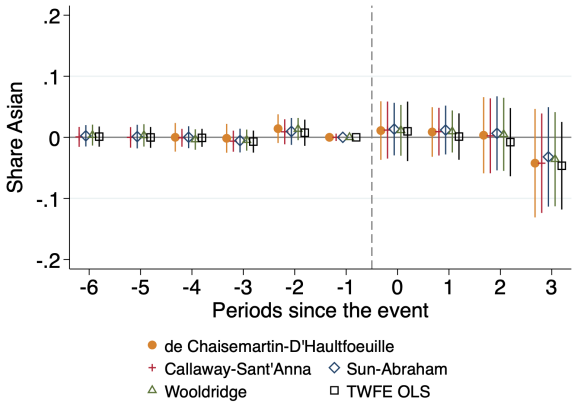
(b) URM Enrollment Shares



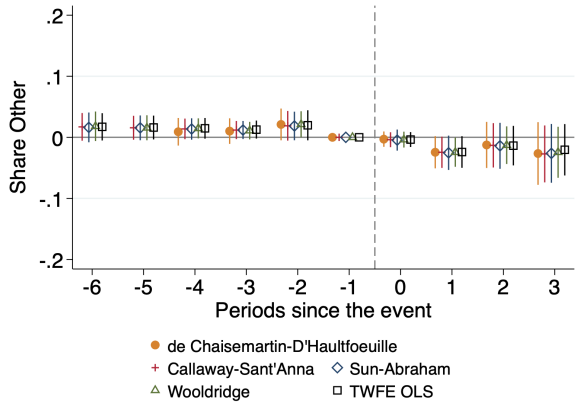
(c) White Enrollment Shares



(d) Asian Enrollment Shares



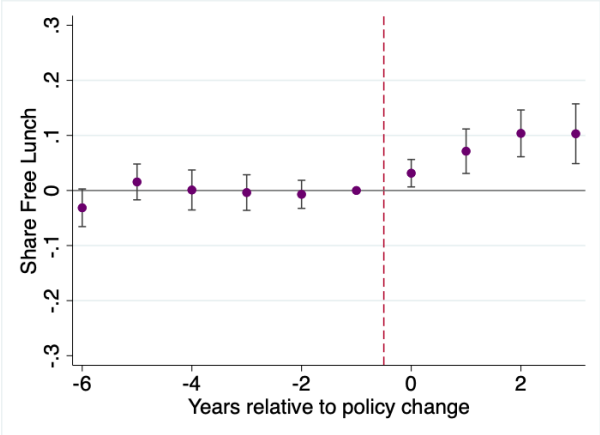
(e) Other Race Enrollment Shares



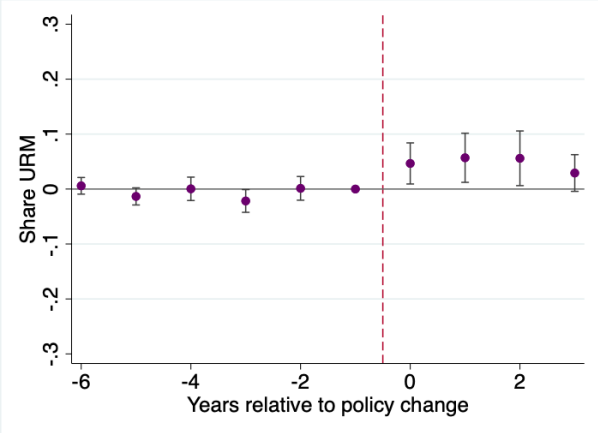
Notes: Each color/shape event study is based on a separate regression estimation (without dose response): black squares present TWFE estimates based on equation 2, orange circles present robust estimates by de Chaisemartin and D'Haultfœuille (2020), red crosses present robust estimates by Callaway and Sant'Anna (2021), blue diamonds present robust estimates by Sun and Abraham (2021), and green triangles present robust estimates by Wooldridge (2021).

Figure A5: Effects on Racial/Ethnic Composition— Test Scores Sample

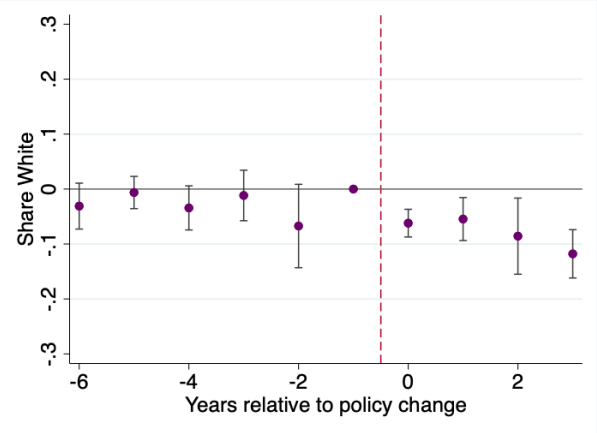
(a) Free Lunch Enrollment Shares



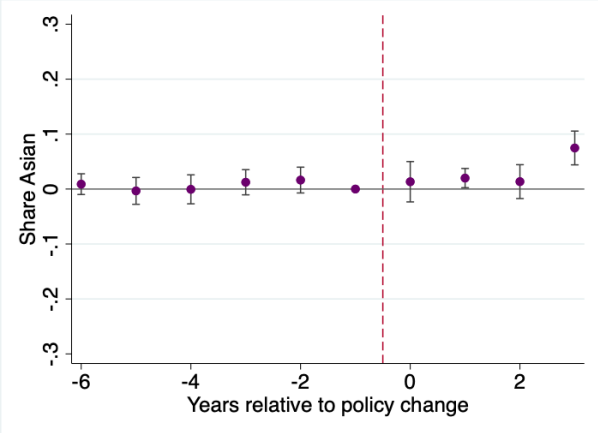
(b) URM Enrollment Shares



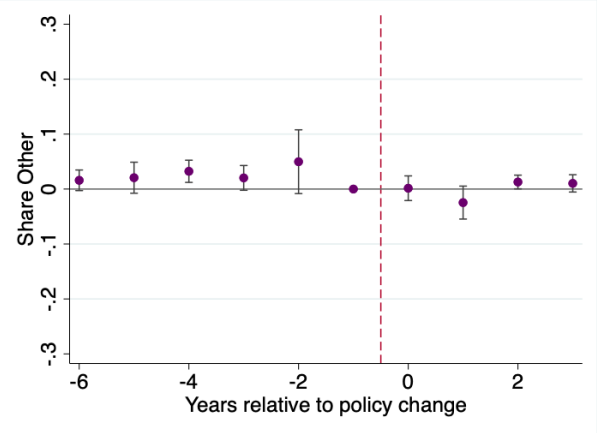
(c) White Enrollment Shares



(d) Asian Enrollment Shares



(e) Other Race Enrollment Shares



Notes: Event studies are based on equation 2. Pre-period trend is estimated and removed from all observations for each school prior to the event study estimation, as described in section 3. For treated schools, this is estimated using all available pre-shock years for each school. For control schools, we use the 2010 to 2015 time-span to estimate the trend. Each event study is based on a separate regression estimation.

Table A1: Effects of SES-based Admissions Policy on Enrollment Shares, excluding or including NYC schools

	Share Free Lunch	Share URM	Share White	Share Asian	Share Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Treated units only include non-NYC academically selective high schools					
SES Policy Change X Post	0.150***	0.075*	-0.045	-0.013	-0.018*
	(0.052)	(0.039)	(0.035)	(0.023)	(0.011)
Percent Effect	36.6%	17.4%	-13.6%	-6.2%	-45.0%
Observations	807	807	807	807	807
Panel B: Treated units only include NYC academically selective high schools					
SES Policy Change X Post	0.122***	0.079***	-0.051*	0.001	-0.028*
	(0.045)	(0.015)	(0.029)	(0.027)	(0.017)
Percent Effect	29.8%	18.4%	-15.5%	0.5%	-70.0%
Observations	919	919	919	919	919

Notes: Estimation is based on equation 1. Panel A excludes treated New York City ASHSs, while Panel B excludes treated non-New York City ASHSs. Percent effect shows the effect relative to the mean of all ASHSs in the final analytic sample from table 1. Standard errors clustered at the school district level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

Table A2: Additional Robustness Tests

	Main Estimates	Pre-2020	Pre-2021	Race Controls	Income Controls	Only Top ASHSs	No Treated District Controls	No 100% FL Years	No 100% FL Schools
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share Free Lunch	0.144***	0.093*	0.133**	0.147***	0.144***	0.164***	0.154***	0.122**	0.096**
	(0.048)	(0.047)	(0.051)	(0.049)	(0.048)	(0.059)	(0.051)	(0.050)	(0.044)
Share URM	0.079***	0.064***	0.070***	0.078***	0.079***	0.091**	0.081***	0.077***	0.076***
	(0.016)	(0.015)	(0.016)	(0.016)	(0.016)	(0.040)	(0.016)	(0.016)	(0.016)
Share White	-0.053**	-0.038*	-0.057***	-0.057**	-0.053**	-0.054	-0.052**	-0.053**	-0.051**
	(0.024)	(0.020)	(0.021)	(0.023)	(0.024)	(0.040)	(0.025)	(0.024)	(0.025)
Share Asian	0.000	0.001	0.011	0.001	0.000	-0.015	0.000	0.002	0.001
	(0.021)	(0.014)	(0.018)	(0.021)	(0.021)	(0.025)	(0.021)	(0.021)	(0.021)
Share Other	-0.026*	-0.028*	-0.024	-0.021*	-0.026*	-0.022*	-0.027*	-0.026*	-0.026*
	(0.014)	(0.015)	(0.015)	(0.012)	(0.014)	(0.012)	(0.015)	(0.015)	(0.015)
N	978	689	775	952	970	811	913	958	901

Notes: Estimation is based on equation 1. (1) Are the main estimates. (2) Excludes all cohorts after 2019. (3) Excludes all cohorts after 2020. (4) Includes controls for yearly changes in 9th-grade racial shares for the entire district. (5) Includes controls for annual changes in 9th-grade low-income shares for the entire district. (6) When multiple ASHSs are treated in a single district, keep only the highest-rated ASHS (effectively drops non-Stuyvesant ASHSs in NYC and non-Boston Latin ASHSs in Boston). (7) Drop all control ASHSs in districts with treated ASHSs from the analysis sample. (8) Drop any observations with 100 percent share of students eligible for Free Lunch. (9) Drop any school that has at least one year with 100 percent share of students eligible for Free Lunch. Standard errors clustered at the school district level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

Table A3: p-values from Permutation Tests—100 iterations

	Share Free Lunch	Share URM	Share White	Share Asian	Share Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Randomly Assigning Policy Dates					
Percent larger than baseline	0.08	0.00	0.01	0.28	0.00
Panel B: Randomly Assigning Policy Dates to Match Actual Policy Date Distribution					
Percent larger than baseline	0.23	0.02	0.06	0.32	0.06

Notes: Refer to section 4.4.4 for details.

Appendix B Policy Changes

B.1 Low-Income Student Quota Admissions Policy

B.1.1 New York City Diversity in Admissions Program (New York, NY)

New York City has sought to increase diversity in its non-specialized schools through prioritization of students with free or reduced-price lunch status. In 2012, PS 133 William A. Butler School in Brooklyn became the first to use this approach, setting aside 35 percent of its seats for FRPL and English language learner (ELL) students.[34]

In the years following the implementation of this policy at PS 133, pressure mounted on the city to make efforts to integrate its public school system. A report from the UCLA Civil Rights Project found that the state of New York had the most segregated school system in the United States.[35] In 2014, a coalition of students founded IntegrateNYC—a non-profit which has since filed a lawsuit against New York City's admissions process.[36] Additionally, the state education department enacted the Socioeconomic Integration Pilot Program (SIPP) which offered up to $10 million from 2015-2018 to support progress toward integration plans and implementation at Title I focus schools in the state.[37]

Beginning with the 2016-17 school year, New York City launched the Diversity in Admissions pilot program at seven schools. Under this program, these schools set aside seats for priority groups such as those eligible for free or reduced-price lunch and English language learners.[38] District leaders were not allowed to explicitly target race in admissions because of the 2007 Parents Involved SCOTUS case. The NYC Education Department followed up the Diversity in Admissions pilot with a diversity plan[39] in June 2017 that set out the city's first publicized diversity goals.[40] One tenet of the city's diversity plan was to expand the Diversity in Admissions program. It proposed outreach to school leadership and communities to spread awareness of the program and its application process.

Building on the pilot program of 2016-17, the Diversity in Admissions program grew from seven to 19 schools for the 2017-18 school year. An additional 11 schools joined the

[34] https://www.dnainfo.com/new-york/20141210/park-slope/how-park-slope-school-is-fighting-stay-diverse/

[35] https://civilrightsproject.ucla.edu/research/k-12-education/integration-and-diversity/ny-norflet-report-placeholder/Kucsera-New-York-Extreme-Segregation-2014.pdf

[36] https://ny.chalkbeat.org/2021/3/9/22321193/nyc-students-file-lawsuit-taking-aim-at-admissions-screens-school-segrega

[37] https://www.p12.nysed.gov/funding/2015-18-title-1-ses-integration-grant/ses-integration-grant.pdf

[38] https://static1.squarespace.com/static/53ee4f0be4b015b9c3690d84/t/5c4f765088251bb7f8e4fd6c/1548711510681/PromisingOutcomes.pdf

[39] https://web.archive.org/web/20170611214002/http://schools.nyc.gov/NR/rdonlyres/D0799D8E-D4CD-45EF-A0D5-8F1DB246C2BA/0/diversity_final.pdf

[40] https://www.nytimes.com/2017/06/06/nyregion/long-awaited-plan-for-integrating-schools-proves-mostly-small-bore.html

program in 2018-19, including the first academically-selective high school in NYC to set aside seats for FRPL-eligible students—Bard High School Early College Queens. The program has continued to grow, with 43 high schools participating in 2023-24.[41] This includes 13 schools that are documented as ASHSs by Finn and Hockett (2012).

Mader et al. (2018) provide a descriptive analysis of the changes in enrollment demographics in participating schools in the first two years of the Diversity in Admissions Initiative (none of the schools during this time were ASHSs). They showed significant increases in low-income enrollment, but no statistically significant change in any race/ethnicity category on average across the pilot schools.

B.2 Eligibility in the Local Context (ELC)

B.2.1 Academic Magnet High School (North Charleston, SC) – Top Two Program

Another class of policy mechanically increases the geographic diversity of admitted students by automatically granting admission to a specified number of students from each middle school in a district. This approach dictates that students from every school have an opportunity to be admitted to the selective high school. Academic Magnet High School in North Charleston, South Carolina implemented this approach through a "Top Two Program" that first applied to the fall 2016 cohort.[42] In this admission system, Academic Magnet High accepts the top two students, by grade point average, from every public middle school in the district. The offer of admission is conditional on the students achieving a minimum score on the school's application rubric which includes grades, standardized test scores, a math task, a writing task, and a teacher recommendation.

B.2.2 Thomas Jefferson High School (Alexandria, VA) – Merit Lottery System

In December 2020, the Fairfax County School Board voted to change the admissions policy for Thomas Jefferson High School for Science and Technology. At this meeting, two alternative methods for selecting students were proposed. First, a proposal to enact a Hybrid Merit Lottery—which would have admitted the top 100 scoring applications, while allocating the remaining 450 seats by lottery among qualified applicants—was rejected. The Board then approved an admissions policy that admits the top 1.5 percent of students from each middle

[41] https://www.schools.nyc.gov/enrollment/enrollment-help/meeting-student-needs/diversity-in-admissions

[42] https://www.postandcourier.com/news/academic-magnet-continues-offering-admissions-to-top-2-graduates-of-middle-s article_65c57dbe-e250-11e6-9a39-171418065599.html

school based on their application scores. This leads to an allocation of approximately 450 seats, with the remaining 100 seats filled by the highest-scoring applicants who have yet to be admitted. This policy has been subject to several recent legal challenges, with the U.S. Court of Appeals for the Fourth Circuit finding in favor of the School Board in May 2023.[43] There have been calls for the case to be heard by the Supreme Court.[44]

B.3 Priority Group Admissions Policy

B.3.1 Liberal Arts and Science Academy (LASA) High School (Austin, TX) – Diversity Process

Until 2017, LASA, located in Austin, Texas, had admitted students based solely on their performance on the school's admissions exam, past grades, an application essay, and teacher recommendations. For the cohort applying for fall 2017 admission, the school considered applicants' race and socioeconomic status for the first time. At this point, LASA shifted to an admissions model in which 80 percent of students were admitted based on their holistic application score. The remaining 20 percent of admitted students are chosen based on their application scores, contingent on meeting at least three out of the five special circumstances defined by the school. These circumstances are being a future first-generation college student, being zoned to a neighborhood middle school with at least 70 percent economically disadvantaged students, belonging to an underrepresented racial or ethnic group, demonstrating academic achievement in the face of difficult circumstances, and embracing academic challenges.[45]

In addition to amending the admissions criteria, LASA also sent out a recruitment team made up of parents of LASA students during the 2017 admissions season. This group met with students at all Title I middle schools in the district and provided guidance to those seeking admission to LASA.[46]

B.3.2 Lowell High School (San Francisco, CA) – Priority Middle School

Lowell High School began its current practice of setting aside 15 percent of seats for students from underrepresented middle schools in 2001. In October 2018, the San Francisco Unified School District Board of Education approved an update to the eligibility criteria under this third band of their admissions system.

[43] https://www.ca4.uscourts.gov/opinions/221280.P.pdf

[44] https://www.nytimes.com/2023/08/21/us/politics/supreme-court-thomas-jefferson-high-school-admissions.html

[45] https://lasa.austinschools.org/acceptance-procedures

[46] https://www.kxan.com/news/lasa-aims-to-diversify-campus-with-new-recruiting-application-process/

Lowell analyzes the enrollment totals at each middle school in San Francisco, total enrollment in the district, and the number of applicants and admitted students from each school over the three preceding years. With these numbers, Lowell identifies a subset of middle schools that are underrepresented within Lowell High School. Under this framework, if an underrepresented school becomes especially proficient at placing students at Lowell, they can lose their underrepresented status. This update identified Willie Brown Middle School as a Band 3 school, regardless of its students' representation within Lowell. The change went into effect for the cohort that entered Lowell for the 2019-20 school year.[47]

It was estimated that the policy would affect 11 current 8th graders at Willie Brown when it was enacted in October 2018.[48] The policy could also impact the composition of Willie Brown as middle school parents respond to the policy. For example, from 2020-2022, the share of Black students at Willie Brown has fallen from 33 percent to 21.9 percent and the share of white students has risen from 10.5 to 18.0 percent. The share of students qualifying for Free or Reduced-price Lunch fell from 66.5 to 52.0 percent.[49]

Lowell instituted lottery admissions which affected the 2021-22 and 2022-23 entering cohorts before returning to selective admissions for 2023-24. Therefore, we only estimate the effects of the 2019 policy change on the 2019 and 2020 cohorts.

B.3.3 Goldstein High School for the Sciences (New York, NY) – Priority for Underrepresented Zip Codes

In 2019, Leon M. Goldstein High School for the Sciences planned to start a Diversity in Admissions Initiative, which would be implemented starting in 2020.[50] Their initiative was to prioritize students from underrepresented zip codes. To do this, they rank order students within each zip code and admit the top ranking students in each zip code, improving the odds of admissions for students who were historically less likely to be admitted.[51]

B.3.4 Boston Public Schools – Socioeconomic Tiers Based on Address

Boston Public Schools operates three academically selective high schools: Boston Latin, Boston Latin Academy, and the O'Bryant School of Mathematics and Science. In 1997, these schools were legally mandated to remove their racial quotas for incoming cohorts. For the 2022 entering cohort, BPS instituted a new admissions system that divides the city's

[47] https://go.boarddocs.com/ca/sfusd/Board.nsf/goto?open&id=AZS28M7BDEA9

[48] https://www.sfchronicle.com/education/article/SF-school-board-changes-Lowell-admission-policy-13294817.php

[49] https://drive.google.com/drive/folders/1MQg9Lrdw0xKxc8FZ8eD2No67kB5P4rYq

[50] https://www.nydailynews.com/2019/10/02/four-nyc-high-schools-adopt-new-admissions-methods-to-increase-diversity

[51] https://www.myschools.nyc/en/schools/high-school/22K535/

Census tracts into eight socioeconomic tiers. Applicants continue to be assessed based on their middle school grades and test scores; however, they are only compared to other students within their tier. An equal number of seats are allocated to each SES level.[52][53]

B.4 Submarginal Conditional Acceptance (SCA)

B.4.1 New York City Specialized High School Discovery Program

The NYC Specialized High School Discovery Program is an alternative admissions option for disadvantaged students who scored within a certain range below the admissions cutoff on the Specialized High School Admissions Test (SHSAT). The main goal of this program is to increase diversity in New York's specialized high schools. Historically, specialized school participation in this program has been optional. A provision in the 1971 New York State law that required admission to the city's specialized high schools to be based solely on the admissions test allowed specialized high schools to sponsor a Discovery Program [54]. By 2015, the majority (5/8) of specialized high schools were still admitting some students through the Discovery Program. The exceptions were Stuyvesant, Bronx Science, and the High School for American Studies at Lehman College.[55][56] At that time, students were eligible for the program as long as they scored within a certain range below the admissions cutoff, were recommended by their middle school, attended and passed a summer preparatory program administered by the specialized high school, and were certified as disadvantaged, based on household income, family receipt of government aid, foster care status, or being recent immigrants who are classified as English language learners.

During the 2016-2017 school year, the NYC DOE released a diversity plan that included their goals and existing strategies to increase diversity in NYC schools. Part of this plan was to expand the Discovery plan. This included restarting the Discovery program at the High School for American Studies at Lehman College in the summer 2016, at Bronx Science in summer 2017, and at Stuyvesant in summer 2018. Additionally, in summer 2016, they expanded the number of Discovery Program students admitted at Brooklyn Tech. Beginning in summer 2019, Discovery Program eligibility was restricted to require that all students have attended a high-poverty middle school in addition to being disadvantaged (low-income family, ELL student recently moved to NYC, and/or student in temporary housing). By 2019-20,

[52] https://www.bostonpublicschools.org/tier-map

[53] https://www.wbur.org/news/2021/07/15/new-exam-school-admissions-policy

[54] https://web.archive.org/web/20140623060948/http://nypress.com/benign-neglect/

[55] https://www.nytimes.com/2018/08/13/nyregion/discovery-program-specialized-schools-nyc.html

[56] Since the early 2000s, Stuyvesant and Bronx Science had terminated their participation in the Discovery Program https://web.archive.org/web/20140623060948/http://nypress.com/benign-neglect/.

20 percent of seats at each specialized high school were reserved for Discovery Program participants.[57]

[57] https://www.schools.nyc.gov/docs/default-source/default-document-library/specialized-high-schools-proposal

Appendix C Addressing Multiple Policy Changes

To identify the effects of specific SES-based admissions policies, we must ensure that we are capturing "clean" policy changes. This mean that if many policy changes are happening at once, it would be difficult to identify which of those policy changes led to the outcome of interest. Therefore, we must deal with issues of this type when we're creating our analytic dataset of policy changes.

Nine ASHSs in Chicago were removed due to transitioning from a race-based affirmative action admissions policy to a place-based admissions policy. While we would be interested in the impacts of the SES place-based policy in Chicago, we believe the termination of the race-based admissions policy contemporaneously impacts admissions in a way that obscures the impact of the SES-based policy.

Additionally, before compiling our data to create Table 1, we removed schools with policy changes in consecutive school years that are impossible to disentangle from each other, as each new policy change may have a new intention/goal. This decision leads us to drop: Academy at Palumbo, Carver High School, Central High School, Julia R Masterman Secondary School, Parkway Center City Middle College, Paul Laurence Dunbar High School, Western High School, and Maggie L. Walker Governor's School.

In some cases, consecutive policy changes have the same intention/goal. We merge policy changes that occur in consecutive school years to be represented as a single policy change in the first year of incidence because all the follow-up changes were made to strengthen the adjusted policy using the same strategy as the initial change. These schools are: McKinley Technology High School, School Without Walls High School, Science Park High School, Baruch College Campus High School, Beacon High School, NYC Lab High School for Collaborative Studies, and School of the Future High School.

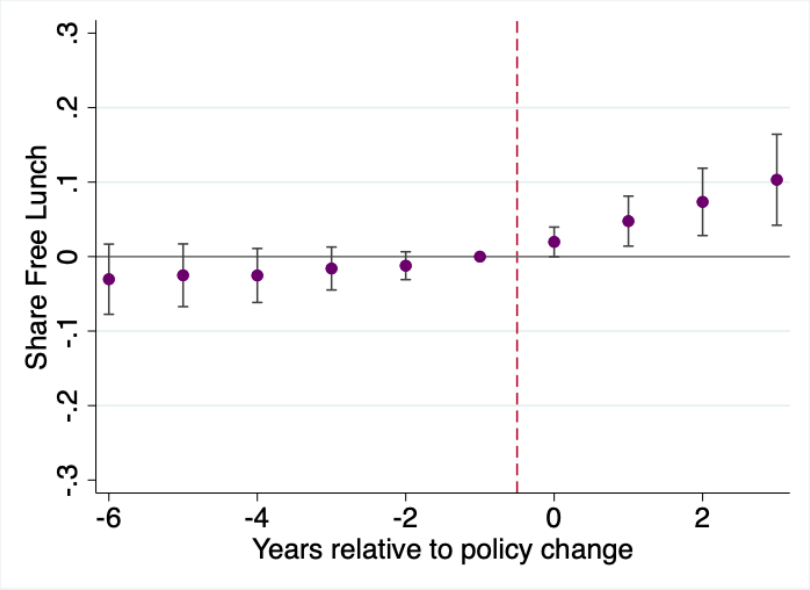
As described in section 2, we further restrict our analytic sample of treated ASHSs to those that are not contaminated by multiple policy changes. We allow ASHSs that had multiple admissions policy changes, as long as they meet our requirement that the treatment timing does not overlap with a previous or future admissions policy change at the same school. That is, we require that the policy change must be at least six years after any previous policy change and at least one year prior to any future policy change. In practice, no school appears more than once in our analysis sample, though many schools have multiple policy changes.

Appendix D Dose-Response vs. Traditional DID and Event Study Estimates

Appendix Figure D1 shows an event study that is analogous to that of figure 4, but without dose-response. The event study shows a small, but immediate, deviation at period zero that increases over time. However, each post-shock coefficient has a different interpretation since only a portion of the school is treated and, hence, must be rescaled for comparability.

Appendix Table D1 shows that DID estimates that do not incorporate a dose-response are mathematically equivalent to those calculated by the dose-response DID approach as long as they are rescaled appropriately. Column (1) shows dynamic DID coefficients estimated in each post-shock period without accounting for dosage. Column (2) shows the proportion of the typical high school that is treated with the additional cohort entering under the new admissions policy. In column (3), we rescale the corresponding estimate by the proportion of cohorts treated, which provides the estimated impact if the whole unit of observation had been treated. In column (4), we show the estimated dynamic impacts from our main approach that incorporates the dose-response. Column (1) and (4) show that both approaches provide estimates with the same level of statistical significance.

Figure D1: Event Study Estimates (without dose-response) – Free-Lunch Enrollment Shares



Notes: Event study is based on equation 2, but without dose-response, therefore $AdmissionsChange_{st}$ is always set to 1 for all treated units. Pre-period trend is estimated and removed from all observations for each school prior to the event study estimation, as described in section 3. For treated schools, this is estimated using all available pre-shock years for each school. For control schools, we use the 2010 to 2015 time-span to estimate the trend.

Table D1: Equivalence between dynamic DID estimates, without and with dose-response

	Estimates w/o Dose-Response	Proportion Treated	Rescaled Estimates	Estimates w/ Dose-Response
	(1)	(2)	(3)	(4)
Panel A: Impact on Free Lunch Enrollment Share				
SES Policy Change X Post0	0.043**	1/4	0.172	0.171**
	(0.020)			(0.082)
SES Policy Change X Post1	0.069**	2/4	0.138	0.139**
	(0.028)			(0.057)
SES Policy Change X Post2	0.099***	3/4	0.132	0.132***
	(0.035)			(0.046)
SES Policy Change X Post3	0.137***	4/4	0.137	0.137***
	(0.045)			(0.045)
Observations	978			978

Notes: Estimates are based on dynamic DID specification, which allows for a separate estimate in each post-shock period. We keep observations with relative time up to six periods after treatment (or after 2016 for control units) in the analysis sample, and we bin relative time periods greater than or equal to four. Refer to section D for details. Standard errors clustered at the school district level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$